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Memoir concerning the Influence which the Nerves of the Lungs possess over the Chemical Phenomena of Respiration. By JEAN MICHAEL PROVENÇAL, Doctor in Medicine of the Faculty of Montpelier, &c. Read before the National Institute of France, Nov. 29, 1809.*

From the New Medical and Physical Journal for 1811.

THE influence exerted by the nervous system upon all the functions of the animal economy is well known; and it was long ago discovered, that if by any accident, the nerves entering into an organ are lacerated, cut, or violently bruised, that organ soon loses the power of sensation and motion, and ceases to perform its proper functions.

Physiologists have known how to profit by these facts which occur in practice; they have performed experiments upon living animals, and purposely divided the nerves in them, in a manner similar to what happens accidentally in men; thus making use of these experiments as an analytical method of studying, in their various relations, the complicated functions of the human system. This, certainly, is not the very best method to follow in studying the phenomena of life; since it is not very easy to

* Journal Gen. de Medicine.

judge of the functions of an organ while it is deprived of nervous influence, and at a time when the animal is disturbed and terrified at the sight of the instrument and distressed by the pain of the operation. It, however, demonstrates sufficiently well the influence of the nervous system on the generality of functions; and if the researches carried on in this way do not always lead to important conclusions, they bring to light certain facts which explain many interesting phenomena. Besides, when we make inquiries into operations so complicated, and into functions so essential to the maintenance of life, it behoves us to consider them under every different point of view, and to employ all sorts of experiments to elucidate their history.

Tying the nerves produces the same effects upon the organs to which they are distributed, as a complete division of those nerves. It destroys all communication between the brain, the spinal marrow, and the organs; impedes the nervous influence, and even intercepts the action of the Galvanic fluid, as M. Humbolt first observed. This philosopher found, that after having cut the nerve of the leg of a frog, and separated it from the surrounding parts, to the extent of a finger's length, if he tied the nerve on a level with the parts on which it was ramified, or, if he covered the portion of nerve included between the ligature and flesh, so that it was not in contact with the air, and afterwards exposed the extremity of the nerve to the galvanic apparatus, he could not perceive in the limb to which the nerve belonged, any effect from the action of the galvanic fluid. Organs destined for any particular secretion, are also affected by tying or cutting the nerves which connect them with the brain or spinal marrow. M. Dumas* observed long ago, that the secretion of the gastric juice was speedily diminished upon cutting or tying the eighth pair of nerves, and also, that when these same nerves were cut or tied, the solution of the aliments was suspended, until fermentation and putrefaction took place.

From the time of Galen, until the present, physiologists have at different epochs, and with various intentions, made ligatures upon, and cut the two nerves of the eighth pair, or sometimes some of its branches only, in order to observe the effects pro-

* Dumas, Principes de Phisiologie, tom. 1, 2d edition.

duced by the interruption of the action of the brain upon the organs to which those nerves are distributed. But no person before M. M. Dupuytren, Dumas, and Blainville, thought of ascertaining what effects dividing those nerves would produce upon the phenomena of respiration, and upon the colour of the arterial blood. M. Dupuytren* first engaged in this inquiry, and in a course of very curious experiments, he observed, that a section of, or ligature upon, the eighth pair of nerves, was always mortal; that dividing them brought on asphyxia; that the arterial blood became dark, and almost of a coal-black colour; that during this asphyxia, the air still continued to enter into the lungs, and the blood to circulate through them; that he could, by means of simple compression of the eighth pair of nerves, bring on asphyxia, and suspending or continuing the compression, could remove the asphyxia, or render it at length fatal. From all these facts, this able surgeon rightly concluded, that respiration in a healthy state is carried on by the influence of the nerves distributed to the lungs, and by that of the nerves of the brain.

M. Dumas, who has enriched physiology and medicine with many valuable experiments and useful discoveries, repeated these experiments, and made some other ingenious ones in order to explain these facts, and he has established the following propositions.

1. The disturbance which pain produces in the respiration is sufficient to change the red colour of arterial blood; it renders it black, in the same manner as dividing the nerves which belong to the lungs, because, in consequence of the derangement produced in these organs by the pain, the air does not sufficiently penetrate them to act upon the blood, and to colour it red.

2. The arterial blood does not become black immediately upon the nerves being divided; it does not take that colour until all the air contained in the interior of the lungs is totally absorbed.

* Expériences touchant l'influence que les nerfs des poumons exercent sur la respiration. Biblioth Méd. 1807.

3. After the nerves are divided, and the red colour of the blood has been changed into black, the red colour may be reproduced, by forcibly introducing into the interior of the lungs, by mechanical impulse, oxygen or atmospheric air.

4. In animals which have had the eighth pair of nerves divided, the appearances do not resemble those produced by suffocation from irrespirable gases, but those arising from a total deprivation of air.

5. By the contact of oxygen with the blood in the arterial trunk the chemical action takes place, which colours the blood red, although this chemical action has not been submitted to the influence of the lungs.

6. The colour of the blood being a physical quality cannot be modified by the vital action in the circumstances essential to its production, but only in the accessory circumstances, such as the introduction and diffusion of air through the vesicles of the lungs, where it is placed in contact with the principles of the blood.

M. Blainville has remarked, that the division of one nerve only of the eighth pair is not mortal; that rabbits die in seven hours, and pigeons on the sixth or seventh day after the division of both nerves; he has observed, that the number of inspirations is diminished after the operation, but that as great a volume of air enters into the lungs of the animal after, as before the division; he found that the arterial blood did not pass into the state of venous blood, and he could not discover any manifest sign of asphyxia. The air inspired, says M. Blainville, appears to be vitiated in the same manner after, as before the operation, whence we may conclude, that the chemical phenomena are not interrupted.

We see by this short account of the experiments of M. M. Dupuytren, Dumas, and Blainville, that these authors do not entirely accord in many points of the history of this experiment, and that every one of them arrives at a different conclusion. My purpose is, not to inquire into the cause of this diversity of opinion, or to endeavour to determine, from their experiments, and those I have myself made, the real phenomena observable in respiration, and the changes the red colour of the

blood undergoes after the section of a ligature upon the nerves of the eighth pair. I propose in this memoir to examine, whether an animal, having had the eighth pair of nerves cut or tied, absorbs as much oxygen, and produces as much carbonic acid after, as before that operation, and if the animal heat undergoes any variation in the course of the experiment. The solution of this important question will serve to complete the beautiful experiments of M. M. Dupuytren, Dumas, and Blainville.

To resolve the first part of the question, I made at Arcueil, in conjunction with M. Berthollet, a great number of experiments on Guinea pigs and rabbits. This was the method I adopted. Before making the ligature or section of the eighth pair of nerves, I placed the animal in a *manomètre** in a given capacity; I ascertained exactly the quantity of oxygen absorbed, and of carbonic acid produced in a given time, while the animal was in a state of perfect health, that I might be able to discover the changes afterward produced in the chemical phenomena of respiration. I then tied or divided the nerves in this animal, and a little time after, placed him in the same manomètre, where I allowed him to remain the same length of time as he did before the operation. I must observe, that the manomètre, I made use of in these experiments, was not provided with a thermometer within side, and that I was obliged to ascertain the temperature before the animal was taken out, by applying the bulb of a thermometer to the manomètre. But this slight inconvenience did not affect the accuracy of the experiment, because it was only necessary to mark the relative quantities. I found the quantity of carbonic acid, by washing the gas with lime or barytes water. All the analyses were made by burning hydrogen gas, after the method of M. M. Humboldt and Gay-Lussac. As it would be tedious and unnecessary to detail every experiment, I have given in the following table the results I have obtained.

* See a description of this instrument by M. Berthollet, in the first vol. of the Society of Arcueil.

Animals submitted to the Experiment.	Time employed in the Experiment.	Analysis of the Air in the Manometre.			
		Parts in 100 of oxygen absorbed.		Parts in 100 of carbonic acid produced.	
		Before cutting the nerves.	After cutting the nerves.	Before cutting the nerves.	After cutting the nerves.
A rabbit	20 minutes	9,86	6,34	6,91	4,62
A rabbit	20 minutes	10,01	6,72	7,80	5,01
A rabbit	20 minutes	9,47	8,02	6,86	5,43
A Guinea pig	20 minutes	7,92	5,81	6,13	3,07
A Guinea pig	20 minutes	7,10	5,98	5,80	4,01

It appears from these few experiments contained in the table, as well as from all the others I have made, that animals, after having had the eighth pair of nerves tied or cut, constantly absorb less oxygen, and give out less carbonic acid than before the operation. This difference in the chemical action of respiration, the effect of the division of the nerves, is at first but little evident, but in proportion to the time elapsed from the operation, the animal always consumes less oxygen, and the quantity of carbonic acid varies in like proportion; and at length, all the chemical phenomena are suspended, cease entirely, and the animal dies.

On opening the body, the lungs are found more or less loaded with black blood. This accumulation does not take place in the last moments of life, as one might suppose; I have opened the thorax of dogs ten or twelve hours after the operation, and while they were yet strong enough to live several hours, whereby I ascertained this fact. It does not take place in animals who die *very shortly* after the section of the nerves, as I have observed in a great number of rabbits and Guinea pigs.

It would certainly be of no little importance, to be able to explain how death takes place in animals in whom the eighth pair of nerves has been tied or cut, and to detail the successive phenomena under which they die; but as this examination would lead me too far from the purpose of the present essay, I shall make that the subject of a second memoir, in which I shall

relate the facts I have observed, in going over the observations and opinions of all the physiologists who have practised this experiment.

As it has been clearly demonstrated by the experiments of Lavoisier and M. Laplace,* that respiration is the principal source of animal heat, and that the temperature of animals depends upon the state of their respiration, it became very interesting to determine if the section of the eighth pair of nerves, which produced so sensible a diminution of the quantity of oxygen which an animal absorbs, and in the quantity of carbonic acid expired in a state of health, would not also produce a difference in the temperature of animals.

In order to determine accurately if the heat of animals underwent any alteration, after the section of the eighth pair of nerves, it was first necessary to ascertain, by a great number of observations, the degree of temperature in the animals destined for experiment, and to compare the results carefully with those obtained after the section of the nerves. But how should we proceed to arrive at an exact knowledge of this temperature? Some physiologists have introduced the bulb of a thermometer into the mouth, ears, and rectum of the animals, whose degree of heat they wish to know. But it appears to me, that this mode of taking the temperature of animals is not very correct; it only gives us the specific heat of the parts themselves. Thus, in opening the mouth of an animal to place the thermometer there, the continual movements of inspiration and expiration, produce in that cavity currents of air which prevent the real temperature being taken. M. Prunelle, in observing the temperature of hedge-hogs, found that the thermometer placed in the mouth indicated the heat to be two-fifths less than the actual temperature of the animal. Other physiologists have applied the bulb of the thermometer to different parts of the body, but this appears to me to be still less exact; for we know, that many circumstances may render the circulation less active towards the external parts, concentrate the vital force inwardly, and speedily change the temperature on the surface of the body.

* Memoirs of the Academy of Sciences, 1781.

The sanguiferous system, and more particularly the arterial canals, being the principal centre of animal heat, it is into one of these large vessels we should plunge the thermometer, if we would ascertain the temperature of animals.

That was the method I employed in all my experiments; it has this inconvenience, that by the speedy death of the animal, the physiologist is prevented from again ascertaining the temperature after the section of the nerves, so as to estimate the change the section has produced in the heat. But as I could easily procure as many dogs as I wished, I remedied this defect by always choosing two dogs of the same size, as nearly as possible of the same age, and of the same sex; I opened the chest of one, and puncturing the aorta near its origin from the left ventricle, I introduced into it the bulb of a thermometer; I then divided the eighth pair of nerves in the other dog, and after some hours, opening his thorax, I punctured the aorta or left ventricle of the heart to ascertain his temperature. I acknowledge, however, that the circumstance of not being able to make both observations on the same animal, leaves some doubt as to the slight modifications the age and temperament of different individuals of the same species might cause in each individual; but I think these modifications are too slight to form any solid objection to the facts I have the honour to lay before the Institute.

I always observed some hours after the operation, that the temperature was less in the dog, in which the eighth pair of nerves had been divided, than in the other dog not operated on. To determine positively whether this diminution of animal heat was at all owing to the wound in the neck, to the accidents occasioned by it, or to the division of the nerves, I exposed to view the nerves in a dog, without dividing them; at the same time, I divided the eight pair of nerves in another dog, and after a certain period I opened them both; I always found that the dog which had merely the nerves exposed by several incisions, was of a higher temperature than the one whose nerves had been divided, and that the heat was equal to that of a dog which had not undergone any operation.

It would be extremely important, to compare these results

with those which would be obtained by making use of a calorimeter. This machine is extremely convenient, and very accurate. One can determine by it with the greatest precision the degrees of alteration the animal heat undergoes from the moment of dividing the eighth pair of nerves, until the animal dies. I shall very speedily make these experiments; I shall compare them with those made by the thermometer, and I shall give an account of them in a work I mean soon to publish, on several matters relating to the history of animal heat. The following table contains the results of my experiments.

Animals opened to ascertain their temperature.	Degrees of the thermometer, (centigrade.)	Animals opened some hours after the division of the eighth pair of nerves, to ascertain their temperature.	Degrees of centigrade thermometer.	Animals opened some hours after the simple exposure of the eighth pair of nerves, to ascertain their temperature.	Degrees of centigrade thermometer.
Ordin. sized dog	39,4	Ordin. sized dog	38,0	Ord. sized dog	40,0
ditto	39,5	ditto	37,2	ditto	39,7
ditto	39,7	ditto	37,5	ditto	40,0
ditto	40,0	ditto	38,1	ditto	39,8
ditto	40,0	ditto	37,0	ditto	40,1
Very large dog	40,0	Very large dog.	37,4	ditto	40,0
ditto	39,9	ditto	37,3	Very large dog	39,9
ditto	40,0	ditto	37,9	ditto	40,0
ditto	40,0	Large pregn. bitch	38,0	ditto	40,0

It appears to me very probable, that the diminution of animal heat, after the division of the eighth pair of nerves, is essentially owing to the alteration produced in the lungs by this division, and particularly to the animal's taking in less oxygen, and producing much less carbonic acid than in a natural state. We know indeed, that dividing the nerves of any muscles will bring on paralysis of those muscles. We are not ignorant that the lungs receive branches from the great sympathetic; but it would appear that the eighth pair exercises considerable influence upon these organs. The lungs, in truth, do not partake of the nature of muscles, but they may be compared to them in respect to the motions they perform, and the necessity of their being supplied with nervous influence, to enable them to exercise their functions properly.

We may, I think, deduce the following propositions from all the facts related in this memoir.

1. Respiration is performed in a natural state, under the influence of the brain, conveyed by means of the eighth pair of nerves.

2. The chemical phenomena of respiration are not destroyed after the division of these nerves; they only become weaker in consequence of the alteration produced in the lungs by this division.

3. Animals, on which this operation has been performed, consume a less quantity of oxygen, and produce less carbonic acid, than in a state of perfect health.

4. The temperature of the dogs opened in these experiments was often above forty degrees of the centigrade thermometer.*

5. When the nerves of the eighth pair were simply exposed to view, the dogs often preserved their natural temperature during the first twenty-four hours.

6. Those, on the contrary, which had the nerves divided, were of a much lower temperature some hours after the division.

A Case of Dysphagia, together with some other unusual Affections, supervening an Inflammation of the Lungs, wherein a Gum Elastic Tube was advantageously employed as a passage to the Stomach. By Edward Barlow, M. D. Member of the Royal College of Surgeons in Ireland, &c.

From the Medical and Physical Journal, for March, 1808.

It frequently happens in the practice of medicine, that a physician's embarrassment arises, not so much from the paucity of his remedies, as from the difficulty of selecting judiciously from among the multiplicity which present themselves to his choice. On the contrary, it but too often occurs, that every means of giving relief are ineffectually exhausted; and that the physician is doomed to experience that keen distress, which must ever attend the consciousness of inability to avert impen-

* 104 degrees of Fahrenheit.—Ed.

ding dissolution. In this latter situation, every auxiliary is valuable; and with a view to extend the knowledge of, and confidence in, an important one, I trouble you with the present communication.

A lady of this city, whose age was 67, was seized, in November, 1805, with symptoms of slight pulmonic inflammation, which readily yielded to blood-letting by leeches, to blisters, and small doses of tincture of digitalis, in conjunction with the other usual treatment. The febrile symptoms had declined, and the several others had gradually subsided. The pulse was reduced from 110 to 86, and perfectly regular; appetite was restored; and a degree of alienation of mind, which had occasionally occurred, particularly during the hot stages of the febrile exacerbations, gave place to perfect intelligence and tranquillity. Under such favourable circumstances, a speedy restoration of health was naturally expected. On the morning, however, of Saturday, the 23d day of November, this being the seventh or eighth day of her confinement, the fourth of medical treatment, she was awakened at four o'clock by a sense of suffocation, and of inability to respire freely. By unaccountable neglect, I was not apprised of the change until twelve o'clock at noon, the hour of visiting, which I had appointed on the preceding evening, when wholly unsuspicious of any sinister event intervening. I found her much distressed by suffocation, but collected in mind, and perfectly adequate both to understand others, and to make herself intelligible, although unable to articulate many words in succession. This inability manifestly proceeded from the muscles of voice being either unequal to exertion, or from some other cause, disobedient to her call; and it is not unworthy of remark, that, both at this time, and throughout her subsequent illness, French words could be pronounced with apparent facility when the correspondent English words could not be articulated. I learned from her, that she suffered no pain; had little or no cough remaining, nor any complaint, save the suffocation which I witnessed. This seemed attended by convulsive catchings of her breathing; the returns of which seemed dependent on the recurring necessities of the lungs for air, which the muscles of respiration, either from quiescence or

irregular action, failed in their agency regularly to supply. The appearance was truly singular; the convulsive catchings being compounded of singultus and dyspnœa, or, in other words, being referable to irregular actions of the ordinary muscles of respiration conjointly with those of the diaphragm. These seemed to fall into irregular actions, not from a recurring disposition to such actions, as is the case in the ordinary instances of clonic spasm, but merely from the demands of the system for a renewal of the air contained within the lungs; for when by such convulsive effort fresh air was inhaled, the muscles seemed immediately to return to a state of perfect inaction, apparently the result of exhaustion and extreme debility, and to continue so until again aroused by a repetition of the same excitement; a term which I here employ solely to express that indirect influence which the necessities of the system appeared, by long established habits of association, to exert over the respiratory organs: by which influence alone these seemed, at this time, to be aroused to any exertion; extreme debility having rendered them nearly incapable of any.

As the period was critical, and the danger manifest, I called immediately for further assistance; and almost instantaneously procured the cooperation of a highly judicious physician, by whom this lady's family had been occasionally attended.

We immediately agreed on the propriety of exhibiting the camphor julep, combined with volatile fetid spirit; and applied a large sinapism, without delay, to the fore part of the chest. By these she was so much relieved, that in the evening she took some bread and coffee with appetite, and seemed nearly free from spasm or suffocation. In order, however, to preserve the ground we had gained, and to guard against a return of complaints, boluses of musk, ammonia, and camphor, were ordered in addition to the former mixture, which was to be continued; due attention being also given to the essential object of nutrition.

This night she slept well, and the next morning (Sunday, 24th November) seemed refreshed. She took breakfast with appetite, but seemed not free from occasional spasms of the organs of respiration; or rather she seemed still unable to em-

ploy the respiratory muscles in that regular and continued exercise essential to free and perfect respiration. Pills were now substituted for the boluses; these being found difficult to swallow. In the evening she grew worse, the suffocation increased, and the cough became troublesome, but without any pain, and to all appearance influenced only by the unexpectorated mucus. A sinapism was ordered, and the pills and mixture were continued. It became necessary too, at this time, to order an enema; and from the tranquillizing effects she formerly experienced from the pediluvium, she was herself desirous of having this repeated, which was ordered accordingly; she also again took bread and coffee with appetite. On the following morning, (Monday 25th) I visited her very early, and found her breathing still laboured, but free from pain; her pulse remained, as throughout, perfectly regular, and of good strength; she had much cough, with free expectoration, which was difficult, however, from weakness and inability to expectorate.

At eleven o'clock, A. M. deglutition became much impeded, and there was a manifestly progressive debility and tendency to spasm, extending upwards to the muscles both of voice and deglutition.

Wine was now administered, with vitriolic ether, and liquid jelly was occasionally given, in order to restore strength by every means. Notwithstanding which, a little after noon, deglutition became entirely suspended; nor could the power be restored, although the external fauces were stimulated, first with cloths wetted with vitriolic ether, and afterwards by warm oil of turpentine and tincture of cantharides. Enemeta too of assafœtida and tincture of castor were injected, with equal inefficacy.

On this evening, there being no amendment, while the strength was rapidly declining, the anxiety of the patient excessive, the throwing about of the arms almost incessant, and, in short, every prospect before us of speedy dissolution, we yet thought it right to procure the assistance of some other practitioner; and a physician of eminence was accordingly called in, by whose advice we put sinapisms to the legs, and blisters to the soles of the feet.

The next morning (Tuesday 26) I was deprived of the aid of that physician, who had attended with me from the onset, he being obliged to leave town. The physician, whom we last called in, however, visited with me at ten o'clock, when we had the mortification to witness a deterioration, in every respect, since the preceding evening. We found the respiration effected only by a species of singultus, and at considerable intervals; the pulse rapidly sinking; deglutition totally suspended; voice nearly lost, being capable only of uttering inarticulate monosyllables; and a countenance expressively indicative of approaching death. Still she remained perfectly sensible, was conscious of her situation, and resigned to the expected event.

Why, under such apparently hopeless circumstances, any further trials were thought expedient, may here require some explanation. In the first place, I was well assured, from the progress of the disease, and the decided concurrence of both my medical friends, that no pulmonic disease existed at this period; and that there was no ailment to contend with, save debility alone. I was satisfied that mere debility need not necessarily destroy life, so long as adequate stimulants could be employed to excite the vital powers to action; I felt that our efforts to introduce both stimulants and nutriment, had been fatally interrupted by the suspension of deglutition; and I decided in my own mind, that if by artificial deglutition a sufficient stimulus could be applied to the stomach, which organ presented the only medium through which the general system could be effectually excited, that life might even yet be preserved. With this view, I had proposed, the preceding evening, the employment of a flexible tube, as a conveyance to the stomach; and had canvassed with that medical friend, whose assistance had been withdrawn from me, the propriety of using such in the present case, and had received his unqualified assent, and warm approbation of the trial, as being the only means which offered any reasonable prospect of giving relief. This proposal I now repeated, and was pleased to find, that the gentleman, to whose judgment I submitted it, did not dissent; although, from the extreme hopelessness of the case, from his being unacquainted with any similar instance, wherein such an expedient was

adopted; and from some apprehension lest the attempt to introduce the tube might bring on general spasm, which might prove fatal, and be the immediate precursor of death, he did not cordially coincide. I had no occasion to dwell with much force of argument on the advantages of introducing food and stimulants to the stomach; but, with respect to the objections, I pleaded that the trial was not a new experiment, as it had been repeatedly made, and with the best effects, in cases of dysphagia; that the absence of every disease, save debility, rendered the present case singularly favourable for such a trial; and, that there was little danger of exciting spasm, either local or general, by the introduction of the tube; for that, in general, it was rather by titillation, than by rougher handling, that the muscles of the fauces were thrown into irregular action; that in cases where pins were swallowed, or other extraneous matter lodged in the œsophagus, surgeons were in the constant practice of introducing *probangs* even as far as the *cardia*, without even vomiting being excited; and further, that there seemed no peculiar disposition to spasm in the present case; inasmuch as those actions, which we witnessed, were not the result of an irritable state of the muscles, so much as of a directly opposite state, a state of atony; for that they were, even with difficulty, excited to perform their necessary and accustomed actions, by the constantly recurring necessity for inspiration, and the occasional voluntary exertions to articulate; whence I inferred, that no unpleasantness of this nature would attend the operation. To all this, my medical friend yielded a reluctant assent; he agreed in its being the only expedient remaining; wished me success; and, by taking up his hat, expressively indicated his intention of not assisting in the trial; and, by this movement, effectually repressed the request for his presence and assistance, to which I was about to give utterance. He took his leave, and left me to the free exercise of my own discretion.

Embarrassing as this situation undoubtedly was, and particularly to a young practitioner, I yet resolved not to abandon the trial, but to give this last and only chance, to my old and very dear friend and relative, whose fate I sincerely lamented.

To this I was even urged by her friends, from their confidence in me, and from the simple explanation I was enabled to give of the proposed expedient. And it may serve to elucidate how far, in similar circumstances, steadiness of purpose, and conviction of the propriety of his measures, on the part of the adviser, may contribute to overcome prejudice or opposition, when I declare, that a ready assent was afforded to my proposition, at a time when by some strange misconception it was even understood, that the gum elastic tube was to be introduced by an artificial opening, instead of by the natural passage.

In defiance of all discouraging circumstances, I was about to proceed to the introduction, when, to my great satisfaction, I received a message from the late Dr. Purcell, whose assistance we had not been able to procure the preceding day, that he was now ready to attend, if required.

I joyfully availed myself of such valuable aid, and had soon the opportunity of detailing to him the history which I have here given. He suggested some trivial remedies; but with a candor, which I shall ever remember, he at once acknowledged, that he could propose nothing so likely to succeed, as the introduction of stimulants to the stomach by an artificial conveyance; and, after listening to my arguments and explanations, he without hesitation agreed to witness and assist me in the trial. I accordingly, under his inspection, introduced the gum elastic tube with perfect ease, and with little disturbance to our patient, while he poured in as much diluted alcohol as he deemed advisable. The quantity, thus introduced, did not I think exceed half an ounce of a mixture containing equal parts of alcohol and water; and this he was unwilling to exceed, especially as he saw with what ease its exhibition could be repeated. After this the abdomen was fomented with warm spirits, and an enema of broth, with opium, was administered. A slight exhaustion seemed to ensue from the trifling fatigue of this inconsiderable exertion, which was soon followed, however, by a general excitement of the whole system, an increase of strength in the pulse, and more regular efforts at breathing. She lay quiet for about an hour, when, from the amendment

throughout her entire appearance, and the total alteration which had taken place therein, I was induced to attempt introducing some warm wine into her mouth, with a view to her swallowing it; and, after a short interval, was delighted to find it actually swallowed, by a perceptible muscular exertion. I continued to give wine by spoonfuls, and was happy to perceive them swallowed with progressively increasing facility; at length, to my utter astonishment, she actually took the cup from my hand, and, by her own effort, raised it to her head, and drank off its contents.

This soon produced some slight aberration of mind, and a degree of incoherence, the manifest effect of a transient inebriety. It soon ceased, however, and on it supervened a sound and refreshing sleep, which lasted several hours, and from which she awoke apparently free from every complaint. She now with firm voice and connected expressions *asked* for wine, which was given her in conjunction with some liquid jelly, and which she took with considerable appetite. Broth enemata were repeated and retained; fomentations were again used, and she again slept soundly. On awaking the second time, the vigor both of mind and body which she evinced, astonished all her freinds who witnessed it. She spoke to them severally, inquired into their health, and seemed highly gratified, both with the attentions she had received, and with the prospect of recovery, of which she had formerly desponded. Her head-dress being completely unsettled, by the jactations of her body during the preceding days, she felt anxious to have it arranged; and, as it was likely to contribute to her better repose, she was indulged. During this short process she displayed a considerable degree of liveliness and humor; and, with a *naiveté* of manner, with which she was singularly gifted, she repeated some well known lines of Pope's, on female vanity, with precision. Her head-dress and bedclothes being settled to her satisfaction, she wished for food; took some biscuit and coffee; expressed a desire to sleep, and, with that view, turned herself on her side, contracting all her limbs into the semiflex position usual with those in perfect health. Her sleep was calm and undisturbed, and continued even to seven o'clock

next morning, (Wednesday the 27th,) when I was again called. Finding, however, that she had taken both wine and jelly during the night, and that a broth enema had also been given and retained, I was unwilling to disturb her, conscious that sleep was more likely to prove restorative than any means I could then employ. I returned again at ten o'clock, when I had to encounter the shock of hearing that all my hopes were fallacious, for that my friend had just expired.

Scarcely crediting the information, I entered, and found the body still warm, with a pulsation distinctly discoverable, both at the heart and wrist. Urged by the suggestions of the moment, I again introduced the tube, and poured diluted alcohol and ether into the stomach.

The stupe too, being in an adjoining chamber, I had cloths wrung from warm spirits applied to the external stomach, the abdomen, and extremities, but in vain.—At length, the spirit was fled, and not to be recalled.

It appeared, on inquiry, that her sleep had for some time become disturbed and uneasy, but her attendants, conceiving it injudicious to awake her, though impelled by their own feelings to do so, had suffered her to continue in this state. A few minutes before ten o'clock she awoke, apparently much agitated; breathed by short and hurried inspirations for a few moments; then breathed her last without a struggle.

It is scarcely required to offer any comments on the facts here stated; it may be right, however, to mention, that they are accurately detailed from notes regularly registered at the time of the occurrence.

The propriety of substituting an artificial mode of deglutition, when the natural had so entirely failed, can admit of little doubt,—and its success, as far as regarded the mere trial, must to all appear complete. That life was not ultimately preserved, in this case, can offer no reasonable objection; for this fatality can only lead us to infer, that, from age, and extreme delicacy of habit, the constitution was here so nicely balanced, so accurately adjusted, as though equal to the preservation of life in health, to be unable to contend with even the slightest degree of disease; and, notwithstanding the removal of disease was survived, yet, that the vital powers, exhausted by the effort,

were incapable of keeping up, by any aid of stimulants, that degree of excitement necessary to the continuance of vital action. Perhaps too, we are called on to admit, that a favourable opportunity for further continuing the necessary excitement was lost, by the timidity of the attendants, who, when sleep became disturbed, and a manifest degree of agitation had commenced, would have acted more wisely by awakening her, and introducing additional wine and sustenance. With respect to the means, by which so much was accomplished, I have no hesitation in recommending its adoption in every case wherein suspended deglutition puts a stop to nutrition and medical treatment; nor do I see why its use should be confined to chronic affections, and not be extended to the latter stages of acute diseases. The expedient is simple in its nature, perfectly harmless, and applicable by any one possessed of the slightest knowledge of the parts concerned; nay, even they who are utterly ignorant thereof, may yet employ it with safety.

Many instances of its employment, in dysphagia, are on record; and it was from recollecting one of these, detailed by the celebrated John Hunter, that I was induced to inforce the use of it, in this case, with so much confidence. The case I here allude to, was one of dysphagia from paralytic affection of the œsophagus; and, from the means thus afforded of sustaining life, and of employing medical treatment, the disease was removed, and the patient ultimately recovered. Of the eligibility of the trial, in the present case, no doubt whatever was expressed, and of its perfect safety I had myself the fullest conviction. I trust it will appear, that I was both warranted therein by previous reasoning, and justified by the result. I further hope, that, in thus giving publicity to this detail, I do no disservice to the practice of medicine.

Conscious of the latitude which private records of diseases give for undetected embellishment, and knowing well with what distrust the unsupported assertions of individuals are oftentimes received by the public, I have deemed it incumbent on me to submit these pages to the inspection of that medical friend, who had witnessed this singular case during a principal part of its progress; and am now authorized to give, in addition to my own authority, the respectable evidence of Dr. Toole,

both as to the authenticity and correctness of most of the preceding statements.

To him I am indebted for much valuable assistance during this trying attendance; and shall be, at all times, happy to express the respect which I feel both for his professional and general character. The latter part of the history, however, must rest on my own credit alone; as the corroboration of those, who witnessed it with me, could not well be asked for; and as the opportunity of adducing a still higher authority, in confirmation of this part of my statement, is unfortunately drawn from me, by the death of the late Dr. Purcell; a man, whose professional eminence was justly merited; whose urbanity of manners, and intelligence of communication, will long be remembered by all who had ever the opportunity of appreciating their value; and whose loss, every admirer of genuine worth and talents will join me in lamenting.

EDWARD BARLOW.

April 7, 1807.

Memoir on the Cause of the Refrigeration observed in Animals exposed to a great heat.

By FRANCIS DELAROCHE, M. D.

Read at the meeting of the first Class in the Institute on the 6th of November, 1809.

Translated for the Eclectic Repertory, from the Journal de Physique.

The animal economy presents some phenomena, which differing in their nature from those observed in unorganized bodies, cannot be explained by the laws of dead matter. It however presents others which resemble more or less known physical effects, and which appear to result from the same laws. Some physiologists, aware of the errors arising from the desire of attributing every thing to mechanical causes, will not admit of any explanation of this kind in the animal economy. They imagine that the phenomena essentially connected with the vital action must depend upon the laws which govern life,

and not on mechanical principles, which have little relation with, and frequently appear opposed to the first: but is not this opinion rather the offspring of speculation than of experience? and because some of the phenomena of life seem incompatible with the laws to which inanimate bodies are subject, must we conclude that all are in the same predicament? This reasoning, faulty in itself, militates against experience. Can any one deny the influence of physical causes, in many of the phenomena of the animal economy, such for instance as distinct vision, which essentially depends on the refractive powers of the humors of the eye; or the mechanism manifest in the motion of our limbs, in which the bones act as levers, and the tendons as cords, &c? It is true that physical causes are not sufficient of themselves without the concurrence of the vital causes to produce these effects.* Their influence is however not the less evident. In general we may pronounce that there is scarcely a phenomenon in the animal economy which may not be referred to one or other of these causes. At one time the influence of the physical causes is the most obvious, at another the vital predominate; and often it is difficult to determine precisely what belongs to the former, and what to the latter. But this is an important object; and the researches conducting to it ought to be ranked among the most interesting in physiology.

If we ever acquire precise ideas respecting the vital causes and the differences which separate them from the mechanical powers, it must be by observing what properly belongs to them in the exercise of life, and not by indiscriminately ascribing to them all the phenomena exhibited by organized bodies.

A case in which it is easiest to make such a distinction is, as it appears to me, presented by animals exposed to a great heat. It is well known that they assume a tempera-

* In speaking of the vital laws and causes, I do not mean to assert, that they are really different or independent of the general laws which govern dead matter. They may be mere modifications. But in the actual state of science they must be admitted if we wish to acquire any tolerably precise ideas of the manner in which the various functions in organized bodies are performed. We are far from being able to refer to physical laws many of the phenomena which these bodies present.

ture much below that of the surrounding medium. For nearly half a century this remarkable property had been observed in animals, and since that time has given rise to many inquiries, and particularly to those made by Sir Joseph Banks, Dr. Fordyce, Sir Charles Blagden, and other philosophers. But as yet no precise ideas have been acquired respecting the cause, which some have ascribed to the cold produced by evaporation of the perspired fluids, while others on the contrary have thought it must be the same as that of animal heat. Some reflections on this question will form the subject of the present memoir; but I will previously state an observation which I made some years ago.* It is that a very exaggerated notion is entertained in supposing that the power of producing cold is as well marked in animals, as that of generating heat. I think I have proved that the opinion generally received since the publication of the experiments of Messrs. Fordyce and Blagden, is erroneous. In a number of experiments made in conjunction with my friend Dr. Berger, I constantly observed that the temperature of animals exposed to a heat above† 95 or 104 degrees, Fahrenheit, rose in a remarkable manner, though it never equalled that of the surrounding medium. I often observed that this elevation of temperature extended 12 or 14 degrees; and I have satisfied myself, that when the external heat is very great, this increase of temperature in the animal has no limits but in death, which necessarily follows. In these experiments I ascertained the temperature of the animals in the most exact manner by means of a thermometer, having a very small bulb, introduced far up the rectum. I have likewise ascertained in man, by a thermometer placed in the mouth, a similar elevation of temperature, and once very decidedly, where the head could not be affected but through the

* In my inaugural dissertation intitled "Experiments on the effects produced by a great heat in the animal economy. Collection of Theses in the Medical school at Paris. 1806, No. 11."

† We have taken the liberty of introducing Fahrenheit's scale, believing that it would be more acceptable to the generality of our readers. Some small fractional differences were unavoidable. Ed.

medium of the circulation; the body being inclosed in a case filled with hot vapors, while the head was exposed to the open air.

Hence it follows, that the power of producing cold is much more restricted, than is generally believed, though it is by no means imaginary. Its existence is attested by too many facts to admit of doubt. It is desirable, therefore, that the cause should be determined, which I shall now attempt.

I have already stated the opinion entertained by some, that this cause must be the same as that of animal heat. It is founded on the experiments of Fordyce and Blagden, from which it might be inferred that animals preserve a uniform temperature whatever may be the heat of the surrounding medium, and that consequently the power of producing cold was as well marked in them as that of producing heat. Indeed, if this were the case, it would be natural to view in this uniformity of temperature, only one and the same phenomenon derived from a single cause. The fact however not being correct, as shown above, the conclusion may be deemed not more so.

This latter opinion receives confirmation from the observation, that in cold-blooded animals the power of preserving a temperature below that of the medium in which they are plunged, when the latter is raised, is as much or more marked than in warm-blooded animals; whereas if this power depended upon the same cause as that of animal heat, it ought to be almost nothing in this class of animals. Indeed I have demonstrated the truth of this assertion by many observations contained in the memoir before referred to. The following experiment, recently made, will, I think, remove all kind of doubts.

I placed a rabbit, whose natural temperature before the experiment was 103 degrees, in a stove at a medium of 113 degrees, Fahrenheit. After remaining an hour and forty minutes, it had acquired a temperature of $110\frac{3}{4}$ degrees. A frog, exposed in the same stove to a similar degree of heat, had acquired, after an hour, a temperature of 80 degrees, which it preserved for half an hour, the remainder of the time it continued in the stove. The temperature of another frog, exposed to a medium

temperature of $115\frac{1}{3}$ degrees, rose to $82\frac{1}{4}$ degrees, where it became stationary.

Those, who consider that there is not a necessary connexion between the cause of animal heat and that of the cold occasionally observed in animals, have been inclined to ascribe the latter to the evaporation from the surface of the body and the interior of the lungs, thus comparing the phenomenon which engages our attention with the refrigeration produced in unorganized bodies having moist surfaces. But is this hypothesis, for which we are indebted to Franklin, founded on fact? The only experiments hitherto made with the view of resolving this question, to wit, those of Blagden and his colleagues, and those of Crawford seem to show that it is not. The experiments I made some years ago, and of which I gave an account in the memoir already alluded to, inclined me to adopt Franklin's opinion, but did not enable me to come to a decided conclusion. I have since attempted new ones, which confirming the result I had formerly obtained, appear to me to remove all doubts on the subject. I shall state the results, promising a short sketch of those I have already published. The latter were chiefly intended to ascertain the validity of the objection pretty generally urged against Franklin's theory, asserting that the refrigeration produced by evaporation was by no means sufficient to explain the difference observed between the temperature of the animals exposed to a great heat, and that of the surrounding medium. To decide this point, it will be sufficient if we examine the comparative influence of heat on the temperature of animals, and that of dead matter the whole surface of which had been moistened. For this purpose I exposed at the same time and together in the same stove several animals, alcarrazas* filled with water and moistened sponges. In making this experiment which I have frequently repeated, I have constantly observed that the alcarrazas and the sponges, whether introduced cold, or previously heated, into the stove, assumed a temperature below that acquired by warm-blooded animals, but nearly the same

* Porous earthen vessels, through which the contained water transudes so as to keep the surface constantly moist. They are used in Spain and other warm climates for the cooling of water.

as the temperature of cold-blooded animals.* From these results we may then infer that evaporation is sufficient to produce a refrigeration equal or greater than that observed in animals; and we may consequently conclude that it is the cause of the latter phenomenon. It would be wrong however to consider the latter conesquence as necessarily following the antecedent. Though a thing be possible we are not intitled to conclude that it actually exists: moreover when I published these results I did not pretend to assert that evaporation was the true cause of the phenomenon before us. I merely offered it as a plausible conjecture. Now I think I can establish it by direct proofs.

If evaporation be the cause which occasions the refrigeration of animals exposed to a great heat, it is evident that by suppressing it on the surface of the body and the interior of the lungs, this refrigeration will be prevented, and the animals must acquire a temperature equal if not superior to that of the medium in which they are placed. If such a result should not take place, it is a direct proof of the insufficiency of this cause. If on the contrary, the means by which the evaporation is prevented, being such as not to disturb the functions of the animal,

* To have rendered the experiment exact, the final temperature acquired by the animals and the inanimate bodies, when the effect of the heat was complete, ought to have been ascertained. This was difficult in warm-blooded animals, a long continued heat exhausting them greatly, I therefore endeavoured to guess at this point. I generally waited till the inanimate bodies had acquired it, which was much easier, as I took care previous to the experiment to raise their temperature nearly to the degree which they would acquire after being placed in the stove. I here present the result of two experiments of this kind lately made.

I inclosed in the same basket, separating them only by a transparent partition, a rabbit and an alcarraza full of water. I placed them in a stove whose mean temperature during the experiment was 113 degrees. The temperature of the rabbit when introduced into the stove, was 103 degrees, that of the alcarraza about 95 degrees. That of the rabbit gradually rose to 110¾ that of the alcarraza on the contrary sunk to 86½ degrees, where it became stationary.

In the second experiment I exposed in the same stove at a mean temperature of 77½ degrees, two small sponges and a frog. The frog placed between the two sponges acquired at the end of an hour a fixed temperature of 82½ degrees, the sponge on the left 82 degrees and the sponge on the right 81½ degrees.

the phenomenon, the object of our inquiry, shall cease, we may fairly conclude that it is owing to evaporation.

This mode of determining the influence of evaporation naturally presented itself to the minds of those who have investigated this subject. Some experiments have been made with this view; but they are neither numerous nor conclusive. One is by Dr. Fordyce. He caused a quantity of steam to be carried into a stove room, and found the heat more disagreeable, but the temperature of his body remained nearly stationary. It is however worthy of remark, that the time he remained in the stove room, was too short to heat in any sensible degree so large a mass as the human body. No positive conclusion, therefore, can be drawn from this experiment. Nor can greater advantage be obtained from that of Dr. Crawford, who endeavoured to ascertain the influence of a hot bath on the temperature of a dog, the method by which he measured the temperature not being exact; and besides the water would only suppress the evaporation from the skin, and not from the lungs. Similar experiments, that he made on frogs in which the evaporation from the lungs is very trifling, would be more conclusive, if the result he announced were confirmed by observation. This however I have found not to be the case. Repeated experiments very carefully conducted have proved to me that frogs uniformly acquire a temperature equal to that of the water in which they are immersed, whatever be its degree of heat, and that in this respect there is no difference between living and dead frogs.*

Such are the experiments, which to the best of my knowledge have been made with the view of determining what would occur in man and other animals exposed to a high degree of heat when no evaporation could take place from the surface of the body. They are evidently insufficient. New ones were required, which I have endeavoured to make.

For this purpose I had recourse to the means employed by Dr. Fordyce, but with this difference, that instead of trying the experiments on man, I selected animals of a small size, so

* See my memoir on the effects produced by great heat on the animal economy. Page 54, and subsequent.

that their bodies might be soon heated. These were well adapted to the end; for it is easy to perceive that if animals are placed in an atmosphere charged with vapor, there will be no evaporation of the fluids exhaled from the surface of the body, nor from the interior of the lungs, and yet their functions will be as freely performed as in a dry air.* The apparatus I used enabled me to distribute the vapors pretty uniformly through the whole space occupied by the animals, and to regulate the quantities at pleasure. I successively introduced different kinds of warm-blooded animals and frogs; I exposed them to different degrees of heat; I carefully examined their temperature, both before and after the experiment, by means of a thermometer, introduced into the rectum or the œsophagus. The result obtained will be found in the following table.†

* An idea will be readily formed of this apparatus by supposing a box of three feet four inches in height, four inches in width, and nearly the same in depth, divided into two chambers by a horizontal railed partition, placed about two thirds from the bottom. On one side of the box there is a door opening into the upper chamber. A little circular wicker work, placed in this chamber, forming a second inclosure in the chamber itself, with a door corresponding with that in the box. In this the animals are placed. A thermometer with a very long tube and small bulb, fixed in the centre of this inclosure, the scale being exterior to the box, indicates the temperature. It is secured from injury by a case of open wicker work.

The vapor is disengaged in a small tin kettle, whence it is conducted by a small bent tube opening into the bottom of the box. A square piece about an inch thick, placed above the bottom serves to break the current of vapor and to distribute it uniformly through the apparatus. Near the lower end of the tube of communication, there is a small stop cock so constructed that the vapour may pass by a lateral opening, by the tube itself, or by both at the same time. By these means the quantity of vapor in the box can be regulated at pleasure, and of course its temperature. This is facilitated by a very simple contrivance, which allows the person to turn the cock, without losing sight of the thermometer, which indicates the temperature of the box. The key of the cock is formed into a pretty long lever, at the ends of which are strings connecting them with the corresponding extremities of another lever of the same length. The latter lever turns on a pivot fixed to the case near the thermometer, and cannot be turned without exciting similar movements in the key of the stop cock.

† Some time ago I made similar experiments and communicated them in a memoir read before the Philomatic society, but not being made with sufficient exactness I declined publishing the memoir, and gave a short extract in the bulletins of that society.

TABLE*

Of the results obtained by exposing different animals to a moist heat, with the view of determining the influence of such heat on their temperature.

Number of the experiments.	Names of the animals.	The time of continuing in the box filled with vapor, expressed in minutes.	Mean temperature of the apparatus during the experiment, expressed in degrees of Fahrenheit.	Temperature of the animal after exposure to the vapor, expressed in degrees of Fahrenheit.	Temperature of the animal previous to its exposure to the vapor, expressed in degrees of Fahrenheit.
1	1st Rabbit	39′	101.6	108.3	104
2	1st Rabbit	55′	101.6	109.2	103.3
3	1st Rabbit	52′	105.2	110.5	104
4	2d Rabbit	55′	101.6	109.2	103.2
5	2d Rabbit	57′	101.6	108.8	104
6	2d Rabbit	55′	105.2	109.3	103.3
7	Guinea Pig	56′	99.9	108.8	102
8	2d ditto	55′	101.6	109.2	102
9	3d ditto	48′ 5″	105.2	110.3	102
10	4th ditto	55′	105.2	111.5	101.1
11	Pigeon	55′	99.9	110.8	108.5
12	Pigeon	40′	105.2	113	$107\frac{1}{4}$
13	Pigeon	42′	$107\frac{1}{4}$	116.4	$107\frac{1}{3}$
14	1st Frog	73′	78	79	
15	2d Frog	50′	81	82	

* I shall offer several remarks on the observations given in this table.

Besides the experiments whose results are here given, I have made several others on the accuracy of which I can rely. Their results have always been analogous to those contained in the table.

Whatever precaution I took there was always some variation in the temperature of the apparatus, during the experiment. These variations did not usually exceed two degrees, but occasionally for a very short time they extended to six degrees.

When any animal was subjected to several experiments, there was always an interval of twenty-four hours between each.

The thermometers used not moving uniformly, I carefully examined this difference, and prepared for each a scale, by which I could reduce to a common standard the different results furnished by these observations. Though in marking the results I have used the tenth of a degree, I do not pretend that the observations were always made with this nicety, which I preferred to committing any voluntary mistake. The errors cannot have exceeded more than the fourth part of a degree.

The temperature of the animal previous to its introduction into the apparatus frequently offered some slight differences, whose cause I never could ascertain.

It was not easy to determine the temperature of frogs immediately on being

In glancing over this table, we perceive that the temperature of warm-blooded animals, is uniformly raised four or six degrees at least above the moist atmosphere in which they are placed. It evidently follows that the power of producing cold had been destroyed, and that consequently this power essentially depends on evaporation. It is true that the heat, to which these animals were exposed, did not exceed their natural temperature more than two degrees; and it might be supposed that in a higher temperature, the power of producing cold would be more distinctly marked. But this objection to the conclusions I have just offered will vanish, when we consider that death would necessarily follow in these animals, on a longer exposure to a moist heat than I subjected them, and that consequently this power would have been extinguished. Indeed, small as may appear the heat which they sustained in these experiments, they were always more or less exhausted; and in those instances where it was greatest, they appeared dying on being withdrawn from the apparatus. The Guinea pig, though very lively in the morning, died the evening of experiment No. 10. A rabbit and a pigeon likewise perished after similar experiments, whose results have not been given in the table.

The question may perhaps be asked, why the temperature of the animals did not acquire an exact equilibrium with that of the surrounding medium, and why did it rise a few degrees higher. The answer to this question is very plain. Their functions not being disturbed, the cause, whatever it may be, that produces animal heat, continued to act, and occasioned the elevation of their temperature. It is more difficult to understand why this elevation was not greater: why the same cause which in low degrees of temperature maintains the animal at 40, 80, or even 160 degrees above the temperature of the surrounding

removed from the box, and without its being affected by contact of the hands or external air. To accomplish it I placed the animal on a small carriage, with the thermometer having a very small bulb resting in the mouth or rather in the stomach. On opening the box, the carriage was rapidly withdrawn, and the degree examined as indicated by the thermometer.

The animals exposed to these experiments were more or less exhausted. When the heat was much increased, they appeared dying at the end of the experiment. The guinea pig was dead at the end of the experiment, No. 10.

medium, should only raise it six or eight degrees, when they are exposed to heat.* This difficulty cannot be resolved till we are able to give a satisfactory answer to the important and frequently disputed question, *what is the cause of animal heat?* A question, according to the facts I have stated, not at all connected with the subject of the present memoir.

The difference between the temperature of frogs and of the surrounding medium, (I believe the same would be true in all cold-blooded animals) has been less obvious than in warm-blooded animals, as might naturally be expected. This led to a very curious remark, which however requires to be confirmed by repeated experiments; namely that the heat of these animals, or the excess of their temperature above that of the surrounding medium, is as considerable when they are exposed to heat, as when they are exposed to cold. Whence it might be inferred that the cause of heat is not the same in them as in warm-blooded animals.

From all that has been stated, it follows that the refrigeration manifested in animals exposed to a great heat ought to be ranked among those phenomena whose cause is essentially physical. It cannot however be denied that the influence of the vital powers, as I have asserted in the beginning of the memoir, concur with the physical in producing all the phenomena which proceed from organization. For, in order that the evaporation producing the cold may take place, it is requisite that the surface of the body and the partition of the pulmonary cells should be constantly moist. But here the comparison between inorganic bodies, and those which were the subject of my experiments ceases. The sides of the former are moistened by simple transudation. In animals they are moistened by perspiration, a function extremely complicated and necessarily dependent on the action of the capillary vessels. In unorganized bodies, when the sides begin to dry, they

* Some new inquiries, which I have made since this memoir was read before the institution, and which I shall shortly publish, induced me to believe that the evaporation was not completely prevented in the experiments just related; but these results do not in any degree invalidate the conclusions I have drawn; on the contrary they rather tend to confirm them.

attract fresh moisture from within: but in animals the perspiration must acquire greater activity, when the heat is raised; and this can only be effected by an increased energy of the exhalent system, or perhaps of the whole circulation. It ought to be remarked that this invigorated activity of the perspiration at the surface of the body, at least is much greater than is necessary to supply the increase of evaporation: hence the sweat, which is nothing more than an excess in the perspired fluids beyond what is carried off by evaporation.

I shall close this memoir by offering the following proposition, which I think I may advance as a necessary corollary from the observations which I have related. The production of cold manifested in animals exposed to a high degree of heat arises from the evaporation of the matter of perspiration, which in proportion to the increased activity of the exhalent system is more considerable as the heat is greater. Hence it is at once the effect of vital and physical causes.

On the Gelatine of the Blood.

BY JOHN BOSTOCK, M. D.
OF LIVERPOOL.

From the Medico-Chirurgical Transactions, Vol. I.

ALTHOUGH different opinions have been entertained respecting the purposes which the blood serves in the animal economy, yet its obvious influence over the functions of life, has, at all times, rendered it an important object of research, both to the physiologist, and the chemist. A complete investigation of its composition and properties is, however, attended with much difficulty: it is a fluid consisting of several ingredients, possessed of qualities peculiar to themselves, and existing in a state of combination, of which we have no other example. According to the latest and most accurate analysis of this fluid, it is supposed to consist of the following parts: fibrine, albumen, gelatine, red globules, soda, some neutral and

earthy salts, a small portion of sulphur, and a peculiar phosphate of iron, all held in solution by a large quantity of water.*

The union of the fibrine and the red globules composes the basis of the crassamentum or clot, which spontaneously forms in the blood, shortly after its evacuation from the vessels. It is to the red globules that the iron is attached; and it seems probable, that from this metallic impregnation, their peculiar colour, and of course, that of the whole mass of blood, is derived. The albumen, the jelly, and the different salts, all dissolved in water, constitute the serum. The characteristic property which the albumen possesses, of being concreted by heat, affords an easy method of obtaining it in a separate state; if, after being rendered insoluble by the process of coagulation, it be cut in small pieces and digested in water, the other ingredients of the serum will remain suspended by the fluid, while the albumen itself is left behind in a state of considerable purity. By evaporating the water we obtain the jelly; but it is unavoidably mixed with the salts, from which it does not appear possible entirely to separate it; this object can only be in part accomplished, by a slow evaporation of the water, in consequence of which, a portion of the salts will assume their regular crystalline form, and may be thus removed from the mass. The small quantity of sulphur, which exists in the blood, appears to be united to the albumen; it has, however, never been obtained in a separate form, and its existence must be regarded as somewhat problematical. That part of the serum, which remains fluid, after the albumen has been coagulated by heat, to which the name of *jelly* or *gelatine* has been applied, is the last of the constituents of the blood, the presence of which has been distinctly ascertained; and it is the one to which in the present paper I propose principally to direct my attention.

In order to give a complete account of the subject, it will be necessary to review the different opinions that have been successively entertained respecting the constitution of the blood

* Parmentier & Deyeux, Jour. Phys. T. xliv. p. 438, 9.
Birkbeck de sanguine, tent. inaug.
Thomson's Fourcroy, V. iii. p. 270, 3.
Fourcroy, Système, T. ix. p. 140.
Thomson's Chemistry, iv. 585 & seq.
Delametherie, sur les êtres, &c. T. ii. p. 148.

in general, the denominations that have been given to its different parts, and the state of relative combination in which they have been supposed to exist. In this review, I shall not entirely confine myself to those authors who have devoted their attention expressly to this subject; I shall notice the opinions of some writers who have only incidentally mentioned it, as from such sources we are often able to ascertain with equal correctness the progressive changes which take place in our knowledge upon topics of this description. After having accomplished this object, in as brief a manner as is consistent with accuracy, I shall give an account of some experiments that I have performed on this part of the blood, which have induced me to adopt an opinion respecting it different from that generally adopted.

We need not be long detained by the opinions of the ancients, on this subject. Although they were frequently accurate in their descriptions of the phenomena of disease, and have manifested considerable diligence in anatomical investigations; of chemistry they were entirely ignorant, and their physiology was so much perverted by preconceived hypotheses, as to have deservedly fallen into complete neglect. According to the opinion of Galen, which, like the other opinions of this celebrated man, was implicitly adopted for some centuries, the blood consists of four parts: blood, properly so called, phlegm, bile, and black bile. Under the first denomination he appears to have included the compound of fibrine and red globules, which we call the crassamentum; the phlegm was used to designate the serum; and the title of black bile appears to have been given to the red globules at the lower part of the clot, which, in consequence of being secluded from the air, had acquired a dark hue. It is not easy to discover to which of the ingredients of the blood the name of bile was attached.*

The illustrious Harvey, who so successfully established the true hypothesis respecting the motion of the blood, appears to have been the first of the moderns who entertained any just

* Galen, de elem. lib. 2. Harvey, de gen. Exer. 52.
Willisii Opera, T. i. p. 66.
Boerhaavii præl. ab Hallero, T. ii. p. 329.
Castelli Lexicon, hæma, arrhus, ichor, phlegma, &c.

conception of the nature of its composition. In his treatise on generation, published in the year 1651, he distinctly speaks of its separation into fibrous and serous parts, as effected by the process of spontaneous coagulation. At the same time he describes a third substance, which occupies the higher part of the clot, and resembles jelly, mucilage, or albumen ovi, to which he gives the title of mucago, and which he regards as the part of the blood the most abounding with spirit. Some authors have supposed, that by this substance he meant to describe the buffy coat, which occasionally appears on the surface of the blood, and others, the substance resembling cream, which sometimes floats on the serum, and was particularly attended to by Hewson and Hunter; but these opinions seem to be scarcely tenable, as this mucilaginous covering is described rather as a constant than as an accidental occurrence. Harvey is supposed to have been the discoverer of the property which the albumen possesses of being coagulated by heat,* and he says, that, by this process, the serum is converted into the peculiar mucilage described above. His claim is rested upon the following passage.—"Et quemadmodum, crudescente san-"guine, parum admodum istiusmodi mucaginis supernatantis "reperitur; ita, si saniem illam a grumo separatam, et effusam "leni foco decoxeris; eandem brevi in mucaginem hanc muta-"tum iri conspicies."—"Quippe urina coctione non densatur "in fibrosum mucaginem, sed potius in lixivium: aquosa au-"tem, sive saniosa hæc pars (sanguinis) aliquandiu leviter "cocta, in mucaginem, innatantem abit:—"†

It must be confessed, that this is not a very accurate description of the process, nor does the term mucago seem at all applicable to the coagulated serum. We may perhaps explain the passage, by supposing, that the coagulation was imperfectly performed, in consequence of the heat either not being sufficiently raised, or not having been long enough applied.

A more accurate idea of the coagulable nature of the serum was entertained by Lower, who, in his treatise on the heart,

* Haller, El. Phys. lib. v. sect 3. § 2.
Thomson's Chem. v. iv. p. 587.
† Harveii Exer. lii. de gen.

published in 1669, describes the liquor pericardii as being similar to the blood, because by the application of heat, it was coagulated and converted into a white jelly.* The fact is mentioned in equally distinct terms by Willis, whose works were published shortly after his death, which took place in 1675. He supposed the blood to contain spirits, sulphur (from which its red colour is derived), salts, earthy matters, and waters; and after a good deal of whimsical hypothesis about the analogy of the blood, first to wine, and afterwards to milk, he notices its division into the red fibrous part, and the coagulated serum. If this latter, he informs us, be exposed to the heat of a fire, it is concreted like the white of the egg, and a coagulum is likewise thrown down from it, by the addition of an acid.†

A considerable advance in our knowledge, respecting the nature of the crassamentum, was made by Malpighi. In his treatise on polypi of the heart, probably written between 1670 and 1680, he combats the idea that was generally entertained, that they consisted of coagulated serum, and gives it as his opinion, that they are formed from the crassamentum of the blood. He informs us, that by repeatedly washing the clot, its colouring matter is entirely removed, and a white substance is left, which, when viewed through a microscope, exhibits a reticulated or fibrous structure, exactly resembling that of polypi. He supposes the buffy coat, ("pellea crusta") which occasionally appears on the top of the clot, to be formed of the same fibrous substance.‡

The knowledge which Borelli possessed respecting the composition of the blood was still farther matured. The albumen and the cruor, the parts into which the blood spontaneously separates, he conceived were themselves compounded bodies, the albumen consisting of a substance coagulable by heat, exactly like the white of the egg, and a watery serum impregnated with salts, while the clot was supposed to consist of a very glutinous substance, united to a purple juice. By repeated

* Lower de corde, p. 6.
† Willisii op. T. 1. p. 72.
‡ Malpighi op. de polypo cordis.

washing, the clot is converted into white fibres or reticulated membranes, while the red matter is carried off by the water.*

The celebrated Boyle wrote his 'Natural History of the Blood' in 1683, two years after the publication of Borelli's work; but although a very elaborate performance, and of considerable length, it cannot be said to have added much to our knowledge respecting the nature of the blood. The attention of the author is almost exclusively occupied with an account of the effects of different chemical reagents upon it, and he relates a number of processes which he performed for the purpose of obtaining, what he calls the spirit of the blood. He speaks of its division into the fibrous and serous parts, and correctly notices the effects of heat, acids, alkalies, alcohol, and the oxymuriate of mercury, in coagulating the serum.

The chemical analysis of the animal fluid was, at this time, pursued with great ardor; but as fire was the principal agent employed, it is not to be supposed that much important information would be obtained. Juncker is supposed to have been the first who analyzed the blood by destructive distillation; he resolved it into "water, spirit, volatile salt, oil, and fixed oil:"† except the water, these were all new products formed during the process. It was about this period that Leeuwenhoek entered upon his laborious microscopical researches into the nature of animal fluids, and among other curious discoveries, detected the presence of the red globules of the blood. He first announced this discovery in a letter to the secretary of the Royal Society, dated August 15th, 1673. Although he appears afterwards to have gone into much unfounded speculation respecting the composition of these globules, their existence has been since amply confirmed, and the discovery must be considered as forming an important step in the progress of our knowledge.‡

It had been long known that the blood contains a saline impregnation; but the first attempt to ascertain the nature of the salts, appears to have been made by Gulielmini, who, by slowly evaporating serum upon glass, obtained them in a crystallized state, and was thus able to distinguish their figure; he acknow-

* Borelli de Motu Anim. T. ii. prop. 132.

† Junckeri, Chymia, p. 75. ‡ Phil. Trans. for 1674, p. 23.

ledges that this method was first pointed out to him by Malpighi.* Gulielmini also paid a good deal of attention to the coagulable lymph, to which he gave the name of "fibra sanguinis." He wrote his treatise on the blood in 1701. Lancisi's learned dissertation on the motion of the heart and arteries was also written early in the 18th century, and contains many observations on the nature of the blood. He directed his attention particularly to the red globules, and controverted the hypothesis of Leeuwenhoek, who conceived that each globule was composed of a series of smaller globules.†

The constitution of the blood formed a very important part of the theory of the celebrated Boerhaave, and he consequently paid much attention to its nature and properties; yet it does not appear that he materially added to the knowledge which was previously acquired upon the subject, and indeed, in some particulars, he seems to have had less accurate ideas respecting it than his contemporaries. Leeuwenhoek's hypothesis of the progressive series of globules, was adopted by Boerhaave in its fullest extent, and formed a fundamental part of his pathological doctrines. From some passages in his writings it may be inferred, that he did not consider the serous and fibrous parts of the blood as essentially distinct from each other, nor from the red globules, but that they all consisted of the same matter differently arranged, and he even speaks of the different parts as being converted occcasionally into each other.‡ Boerhaave entered upon the discussion of the question, which was at one time much agitated, whether the blood was acid or alkaline, and from finding that no effervescence was produced by the addition of either acids or alkalies to it, he concluded that it possessed neither of these qualities.§ It is, however, now known that this conclusion was erroneous, and we are able to perceive the source of the error into which he was betrayed.

We may notice the account that Hoffman gives of the blood,

* Guliemini Opera. T. ii. sect. 44.

† Lancisi, Opera. T. iv. de motu cord. post xvi.

‡ Boerhaave, aphor. sect. 93, 94.

———, prælect. sect. 223 & not.

§ Boerhaave, chem. T. ii. proc. 114.

rather in consideration of his general celebrity, than from any peculiar sagacity which he displayed upon this subject. Blood, he supposes, is composed of watery, sulphureous, and earthy parts, that it is nothing more than a purple jelly, although it differs from common jelly in being more inflammable, in yielding more volatile salt and oil when distilled, and in being more fetid when it putrefies. He concludes therefore that blood is a jelly of a more concocted or subtile kind.*

The next account of the blood that we meet with is in Senac's elaborate dissertation on the heart. He describes in detail its different constituents, with considerable minuteness; beginning with the red globules, he afterwards proceeds to what he calls the lymph, then the gelatinous matter, the mucus, and lastly the serosity. By lymph he certainly meant to designate the part which we now call fibrine; he, however, appears to have had no very accurate conception of the difference between the fibrine and the albumen, as he says the lymph may be hardened by exposure to heat; and he also supposes that the buffy coat of inflamed blood is composed o f albumen. With respect to the jelly and the mucus said to be in the blood, he seems rather to have inferred their existence from analogy, than to have actually detected their presence. He compares the jelly of the blood to that which is found in broth, and says, that it may be obtained from the blood by boiling it. The existence of mucus he infers, from the quantity of this substance which is poured out on the secreting surfaces with which the body abounds; conceiving it more probable that it should have been ready formed in the blood, than generated by glands lying contiguous to the parts. By the serosity Senac unquestionably meant the serum; he says it is coagulated by heat, and resembles the white of the egg; and he points out the particulars in which it differs from jelly.† As far as I have been able to collect, Senac is the first writer who employed the words coagulable lymph and serosity; he used the first in the same sense in which we now employ it; but he certainly considered serosity as synonymous with serum, and he does not appear to have had

* Hoffman, Rat. Med. lib. i. sect. 1. cap. 5.

† Senac traité du cœur, liv. 3. chap. 4.

any idea of the existence of that part of the blood to which some late writers have applied the term.

Not long after Senac, De Haen published some remarks upon the blood. He points out the method of obtaining the fibrine, or as he styles it, the membrane of the blood, by rapidly stirring it, when fresh drawn, with a stick (a process which he informs us was first practised by Ruysch) or still more readily, by agitating the blood in a bottle. After noticing the resemblance of the serum to the white of the egg, he informs us, that if blood be suffered to flow into warm water, a substance is procured from it which he calls "gelatum." This was probably a thin film of coagulated fibrine; it certainly could not have been what has since been called the jelly of the blood.

As M. Fourcroy refers to De Haen, as the original observer of the gelatine, I shall quote at length the passage in which he describes this peculiar substance.

"Sanguinem alium ex brachio, alium ex pede effluentem, in "aquam ad 100 gradus thermometro imposito calentem, ex- "cepi. Sanguis ille observatur primo hanc acquam æquabili "rubedine tingere, si jactus ejus citatus est: refrigeratæ dein "aquæ innatat plurimum albescentis pellucidi, glutinosi, fugi- "entis ad tactum, manu, aut cochleare vix capiundi; hæc si "brevi a venæ sectione rem explores. Si vero per 8, 10 horas "aqua illa post venæ sectionem sine examine reponatur, revo- "ceturque ad examen, habebit nonnunquam parum illius pellu- "cidi glutinosi, sed multo solidius, colore fuscum, facilius capi- "endum. Hoc contentum quo scirem quid esset, pluries illud "charta emporetica, linteo suffulta, percolavi: nonnunquam "remansit nihil; aliquoties quid pauci, glutinosi, quod exsicca- "tum furfura, griseive pulveris instar, in spiritu vini perstitit. "Materies hæc glutinosa, ex aqua adhuc calente, mox a venæ "sectione exceptæ, et vel ramulo, aut lagena, agitata, iteratis "experimentis nihil unquam dedit membranosi. Si experi- "mentum instituis longè post venæ sectionem, ubi aliquoties "contenta materies solidior apparet, est tamen idem experimenti "effectus. Hæc porro solidior materies, ab ea, quæ ex calente "adhuc aqua eximitur, differt in eo, quod percolata plus relin- "quat materiæ glutinosæ fuscæ. Hanc superstitem a percola-

"tione materiem per horam agitavi lagena, visurus num mem-
"branam daret? Nequaquam; agitatio illa materiem modo ex
"fusco reddidit rubicundiorem. Percolata omnium horum ex-
"perimentorum aqua rubella est, et mucosi quid in fundo
"habet. Bidui spatio omne id mucosum fere evanescit in aqua.
"Experimenta hæc sæpius, & in phlogistico sanguine, & in
"sano, capta sunt. Gelatum ergo illud, quod in aqua calida a
"misso sanguine colligitur, neque materies factitiæ membranæ
"est, ut mox ostendi, neque etiam est crustæ phlogisticæ ma-
"teria. Est enim gelatum hoc in quavis aqua, in quam calen-
"tem sanguis sive ex brachio, influxerit. Saltem si esset
"crustæ materies, ii, quibus neque a morbo, neque a gravitate,
"neque a diathesi crusta adesset, gelati carerent."*

Our knowledge respecting the blood was in this state, when in the year 1759, the celebrated Haller published the second volume of his Elements of Physiology, in which he treats with considerable minuteness, of the nature and properties of the blood. He details the opinions of many of his predecessors, and discusses the various controverted points with much of his accustomed candor and learning. We cannot, however, but remark, that this illustrious physiologist entertained less correct notions about the blood, than might have been expected from one possessed of his extensive information and indefatigable research. After noticing the separation into crassamentum, or as he calls it, cruor, and serum, and describing the former of these substances, he devotes a section to the particular examination of the latter. He supposes that it consists of water, mucus, and jelly; but it appears evident from several parts of his work that he did not employ the term jelly in that confined sense to which it is restricted by the accuracy of modern chemistry. He seems not to have been aware of the distinction which exists between the albumen of the blood, and the jelly which is obtained by boiling the membranous parts of animals; he also confounds it with the exsudation which proceeds from wounded vessels, and which closes up the mouths of arteries, and he even imagined that polypous concretions, and the buffy coat of inflamed blood are formed of albu-

* De Haen, Rat. Med. pars 1. cap. 6.

men. Hence we may safely conclude, that, although he enumerates jelly among the constituents of the serum, he was not acquainted with the substance to which the term gelatine has been since applied by Fourcroy and other later writers; there is no evidence of his having obtained it in a separate state, or even having been aware of its existence; and he evidently uses the word jelly to describe a substance of very different properties. By the mucus of the blood, Haller meant to speak of that substance which was obtained by De Haen, by permitting the blood to flow into warm water; for the proof of its existence, he refers to the passage which I have quoted above from this author. I do not find that the word serosity is mentioned by Haller in his Elements of Physiology.*

From the account which I have given of the authors who have treated of the subject before this period, we may conclude, that the existence of any animal matter in the serum, distinct from the albumen, and not coagulable by heat, was unknown to them; much less was there any idea entertained of the serum containing a proper jelly. When this word was employed by these writers, to designate any of the constituents of the blood, they used it in a vague sense, generally applying it to the serum, but sometimes to the whole mass of the blood. We may remark that the word jellying, or some synonymous term, is frequently employed to describe the process of coagulation, both the spontaneous concretion of the crassamentum, which takes place when blood is drawn from the vessels, and the consolidation of the albumen, which is effected by heat. Of this we have the most striking illustration in the works of Haller himself, who intitles one of his sections, "Seri pars quæ cogitur, vel gelatinosa."

The publication of Haller's great work may justly be considered as forming an important era in the history of the science; by rendering information more accessible, it tended materially to the diffusion of physiological knowledge, and thus immediately prepared the way for many important improvements. After having ascertained the state of the opinion that was en-

* Haller, El. Phys. lib. 5. sect. 3.

tertained respecting the serum of blood at this period, I shall now proceed to trace the different changes which it has undergone until the present day.

The first clear notification which I have met with, of an uncoagulable animal matter in the serum, is in an inaugural dissertation published at Edinburgh in 1760, by Dr. Butt. After remarking the separation of the blood into serum, and crassamentum, he observes, that serum consists of two parts, a coagulable substance, and a watery fluid, which last also exhibits marks of containing some animal matter. Dr. Butt fell into the inaccuracy, which was common among his contemporaries, of considering the albumen and coagulable lymph not to be essentially different from each other; he expressly states, that he regards the white of the egg as nothing more than coagulable lymph in its purest form.* No notice is taken of this peculiar part of the serum by Gaubius, who published his pathology in 1763; indeed he so little understood the nature of this fluid, as to conclude that its glutinous texture depended upon a mucilaginous matter, resembling vegetable gum.†

It was three years after this period, that the celebrated Cullen first gave medical lectures at Edinburgh; and I believe, it was about the same time, that he published, for the use of his class, the text book intitled "Institutions of Medicine," in which we may observe some farther advances in our knowledge respecting the constitution of the blood, although the account which he gives is still embarrassed with much inaccuracy. What had been called the fibrous part of the blood by Malpighi, Gulielmini, Gaubius, and others, and by Senac the coagulable lymph, Cullen denominated gluten. He observes, that when the serum has been coagulated by heat, if it be cut into small pieces, a thin fluid, of a saline taste, exudes from it; to this fluid he gave the name of serosity; and he considered the serum as formed merely by the solution of a quantity of gluten in this serosity.‡ Hence it appears, that he had not a distinct conception of the difference between the albumen and the gluten, nor of their re-

* Butt, p. 53. & alibi.

† Gaubii Inst. path. sect. 337, & seq.

‡ Cullen's Instit. of Med. sect. 247. & seq.

lation to each other. We may remark also, that the sense in which he employed the word serosity, was very different from that given to it by its original inventor Senac; yet the wide diffusion which the opinions of Cullen would acquire, in consequence of his situation as a public teacher, necessarily gave them a great degree of currency; and we accordingly find that, from this period, the word serosity was generally restricted to the uncoagulable part of the serum.

In the year 1770, Mr. Hewson first published the result of his inquiries into the nature and constitution of the blood; some of his opinions, particularly those respecting the red particles, are now discarded, as being without foundation; but on many points he will be found to be considerably more correct than any of his predecessors. After remarking that the coagulable lymph and serum had been confounded with each other, even by the best informed writers, immediately preceding the period when he wrote, he proceeds to notice their distinctive characters, and after describing the coagulation of the latter substance, observes that a watery fluid may be pressed out of it, which he calls the serosity. This fluid, he says, contains the neutral salts of the blood, and also a mucilage which cannot be coagulated by heat; but if part of the water be evaporated, it then acquires a firm consistence, and resembles the mucus spit up from the lungs, when dried.* Mr. Hewson has indeed fallen into an error, in supposing that this peculiar fluid has the name of serosity given to it by Senac; this author, as we remarked above, seems to have been the first who used the word, but he applied it to the serum at large, while Cullen restricted it to this particular part of it. Gaber, who performed his experiments on pus about the time of Mr. Hewson's publication, and who imagines that purulent matter was formed from what he calls serosity, evidently intended, under this denomination, to speak of the serum at large.† Dr. Gregory, on the other hand, in his "Conspectus," uses the word serosity in the same sense with Cullen.‡ I do not find that this part of the blood is noticed by Mr. Hey in his "Inquiry."

* Phil. Trans. 1770; & Inquiry, passim.
† Journ. de Phys. Introd. T. ii. p. 23.
‡ Gregory, Conspect. T. i. sect. 502.

No particular alteration appears to have taken place in the opinions respecting the composition of the blood until the year 1790, when the following discovery was announced to the academy of sciences by MM. Fourcroy and Vauquelin. "If "serum be exposed to heat, after being mixed with half its "weight of water, it is in part coagulated; and the portion of "liquid which is not coagulated contains gelatine, which gelati-"nizes by cooling."* The discovery was developed more at large in a future paper, in which the authors inform us, that a slightly turbid fluid may be separated from coagulated albumen, which, by evaporation and cooling, concretes into a substance possessing every characteristic of true jelly.† They state that this substance had been seen by De Haen, though without referring to any particular part of his treatise; but from a careful perusal of it, in connexion with the references made to it by Senac and Haller, I believe there can be no doubt, that it is the passage which I have quoted above that contains the supposed discovery of De Haen. I have already stated, that I am disposed to draw a different conclusion from it.

A still more particular account of this substance is contained in a paper published by MM. Parmentier and Deyeux in 1794. They appear at first to have entertained some doubt about the accuracy of M. Fourcroy's conclusion, and took considerable pains to ascertain the properties of this supposed gelatine. Their experiments led them to be fully satisfied as to its existence; and they relate with minuteness, both the process which they employed to procure it, and the nature of the substance which they obtained. The properties, however, which they ascribe to it are neither essential to, nor characteristic of, jelly. They do not state that it is capable of being concreted by cold, nor do they notice the effects of any decisive chemical tests; they inform us that it is glutinous to the touch; and that when dried, it composes a hard transparent film.‡

* Ann. de Chimie, T. vi. p. 182.

† Mem. Acad. Scien. 1789, p. 297.
Ann. de Chimie. T. vii. p. 146.

‡ Journ. de Phys. T. xliv. p. 438, 9.

At the same time, that the French chemists were thus occupying themselves with detecting the properties of this ingredient in the blood, the celebrated John Hunter, ignorant probably of their operations, was likewise engaged in examining the same substance. He describes the serum of the blood as consisting of two distinct fluids, one coagulable by heat, like the white of the egg, and the other, which remains uncoagulated. He discovered that this peculiar part of the serum is precipitated by Goulard's extract; and in this manner he attempted to ascertain the relative proportion in which this substance and the albumen of the blood existed in the blood of different individuals, and in other serous fluids which he examined. This method, however, as will afterwards appear, is totally inadequate to the purpose. None of the properties of this substance, as pointed out by Mr. Hunter, are at all analogous to those of jelly, nor does it appear that he, in any respect, considered it as a gelatinous fluid.*

Since this period no material change seems to have taken place in the opinion entertained respecting the constitution of the blood, at least with regard to the existence and properties of the gelatine. Those writers who have treated the subject the most copiously, Dr. Birkbeck,† Mr. Allen,‡ M. Dumas,‖ M. Fourcroy,§ Dr. Thomson,¶ and M. Delametherie,** agree in representing one of the constituents of the blood to be a proper jelly, which is liquified by heat, and congeals again by cooling; and the same opinion is maintained by Professor Blumenbach,†† Mr. Hatchet,‡‡ and M. Richerand,§§ who incidentally mention the subject. The only author of respectability, who supports a different doctrine, is Mr. John Bell; but I conceive that a perusal of his remarks on the blood will prove, that

* Hunter on the Blood, p. 32, 3.
† Tent. Inaug. de sanguine, passim.
‡ Thomson's Fourcroy, T. iii. p. 270—3.
‖ Dumas, princip. de phys. T. ii. p. 37, 8.
§ Fourcroy, systeme, T. ix. p. 140.
¶ System of Chemistry, V. iv. p. 585. & seq.
** Delametherie, Considerations, T. ii. p. 148.
†† Blumenbach, Inst. Phys. sect. 11.
‡‡ Phil. Trans. 1800. p. 401. §§ Richerand, Elem. Phys. p. 184.

notwithstanding his acknowledged talents, and the acuteness with which he has detected the mistakes of others, he has himself, on this subject, betrayed a great deficiency of information. He says that blood consists of crassamentum, serum, and red globules, forgetting that the red globules are one of the component parts of the crassamentum; he uses the terms gluten and jelly as synonymous, and expressly states, that all attempts to distinguish between the glutinous and albuminous parts of the blood are vain and useless. Although he frequently employs the word jelly, when speaking of the constituents of the blood, it does not appear that he had any distinct conception of that part of the serum which we are now describing; indeed the whole section is so confused, that it is not easy to develop the author's exact meaning.*

Having thus completed a sketch of the opinions that have been entertained by others upon the subject, it remains for me to give an account of my own, and especially to point out the circumstances in which they differ from those of my contemporaries. Before, however, I can do this, I shall be under the necessity of adverting to some experiments that I have lately made on the analysis of animal fluids, and on the method of discriminating between those which the most nearly resemble each other in their visible properties, and which, in consequence of their being found in greater or less proportion in almost all parts of the body, I have denominated primary. Of these there are three, albumen, jelly, and mucus. The distinguishing characters of the first are its being coagulable by heat, and by the oxymuriate of mercury. The second is liquified by heat, and becomes solid again by cold: it is not affected by the oxymuriate of mercury, but is precipitated from its solution by tan; while mucus is neither coagulated by heat, nor has the power of concreting by cold, it is not affected by the oxymuriate of mercury, nor by tan; but it is copiously precipitated by the acetate of lead.†

In order to ascertain the nature of the uncoagulable part of the serum, I exposed a quantity of it for some time to the heat

* Anatomy, V. ii. p. 87, & seq.

† Ed. Med. Journ. V. i. p. 257. Nicholson's Journ. V. xi. p. 244.

of boiling water; it concreted, in the usual manner, into a solid mass; but upon being divided into small pieces, and laid upon an inclined pane of glass, a brownish liquor oozed from it. The pieces of serum were afterwards digested in boiling water, which became tinged of a brown colour, owing to some substance previously contained in the serum which it had carried off. The fluid which oozed from the coagulated serum, and the water in which it had been digested, were added together. To a portion of it a small quantity of the solution of the oxymuriate of mercury being added, it became milky, and a precipitate was formed; it was also rendered opake by being for some time exposed to the boiling temperature. Hence I found that it still contained some uncoagulated albumen; and in order more effectually to separate it, I diluted a quantity of serum with six times its bulk of water; to this I added the solution of the oxymuriate of mercury, until no farther precipitation could be perceived, and placed the compound in the water bath. The coagulum was by this process rendered considerably firmer than when heat only had been employed, and the liquor remained nearly transparent; it was passed through a filtre, and now no precipitate could be obtained by the addition of the infusion of tan. A quantity of the water in which coagulated serum had been digested was slowly evaporated; when the greatest part of the water was separated, it was suffered to cool, but no appearance of gelatinization was perceptible. The evaporation was then continued to dryness; a tenacious film of animal matter was left behind, which did not in any respect resemble dried jelly, and which was with difficulty redissolved by the addition of more water. These experiments were several times repeated; and the results were essentially the same so far at least as affected the conclusion to be drawn from them. It is necessary however to remark, that in trying different specimens of serum, there was a considerable difference perceptible in the readiness with which the albumen was separated from the uncoagulable part; in some instances a single operation was sufficient, while in others it was necessary to repeat the addition of the oxymuriate of mercury and the boiling four or five times, until the liquor which was passed through the filtre was entirely freed from the uncoagulated albumen.

From these experiments I felt myself justified in concluding:—First, That when diluted serum is completely deprived of albumen, which is proved by its no longer yielding a precipitate, upon being boiled with the oxymuriate of mercury, it is not affected by the infusion of tan. Secondly, That the animal matter contained in serum, which is not coagulated by the operation of heat or the oxymuriate of mercury, does not possess the property of concreting by cold. Whence we may infer, in the third place, That that part of the serum which is not coagulable by heat does not possess the properties which are essential to jelly, either physical or chemical.

Having thus found that the uncoagulable part of the serum is not jelly, I was induced, from my ideas of the constitution of animal fluids, to consider it as consisting of mucus. I have not indeed been able unequivocally to establish this opinion; for although the water, in which the albumen had been digested, was very copiously precipitated by the acetate of lead, yet it might be conceived that this effect was produced by the decomposition of the different saline bodies that exist in the blood. I however thought myself authorized in considering it to be a proper mucus, both from the nature of the precipitate produced by the acetate of lead, which exhibited the peculiar flaky form, which is indicative of an animal or vegetable impregnation, and likewise from observing the appearance that it assumed by evaporation. Before I leave this part of the subject I may observe, as a confirmation of the inferences deduced from my experiments on serum, that I have had an opportunity of examining the fluids from a tumor on a diseased spine, from a case of hydrocephalus internus, and from a hydrocele, and that I could not in any of them detect the least trace of jelly.

As the opinion which is entertained, by authors of the first respectability, respecting the uncoagulable part of the serum, appears to be incorrect, it may be thought incumbent upon me to point out the circumstances which have contributed to establish the erroneous doctrine. In the first place we may observe, that a considerable degree of inaccuracy pervades the language of even the most correct writers on subjects connected with

animal chemistry. The terms jellying or gelatinization, which ought to be restricted to the property that heated jelly possesses of becoming solid by cold, have been applied to every case in which a fluid substance is converted into the concrete state; whether by heat, as in the instance of the albumen, or by what has been called spontaneous coagulation, as is observed in the fibrine. This inaccuracy may probably, in the first instance, have misled M. Fourcroy, who finding these expressions employed by writers of high reputation, as applied to the constituents of the blood, entered upon his experiments with his mind biassed in favour of the idea, that he must meet with jelly as one of its component parts. When, under this impression, he instituted his experiments upon the uncoagulable part of the serum, it is easy to conceive, that he might mistake the effects of desiccation for those of gelatinization; and as far as appears, he did not employ any other method of ascertaining the nature of the substance, than the change produced in it by the effect of heat. Indeed, although he states the fact in different parts of his works with perfect confidence, and claims the discovery, as one which he thought of importance, he no where gives us any account of the manner in which his experiments were conducted. The subject was treated much more in detail by MM. Parmentier and Deyeux; but although they conclude in favour of M. Fourcroy's opinion, we shall not find that their results authorize this conclusion. They do not say that the substance, which they obtained, concreted by cold; nor do they seem to have thought of examining its nature by any chemical tests; the properties which they point out are by no means characteristic of jelly.

As to Mr. Hunter's experiments, it is obvious that they rather confirm, than oppose, the opinion that I am desirous of establishing. He found the part of the serum, which is not coagulable by heat, to be precipitable by the acetate of lead, a substance, which is the appropriate test of mucus, and has no action upon jelly. He does not appear to have examined the effects of heat upon it. The authors who have more recently treated upon the subject appear, at least for the most part, to have taken up the opinion of the French chemists without

farther examination; and indeed, after the very direct manner in which it had been stated, we can scarcely accuse them of rashness or improper confidence. Both Dr. Birkbeck and Mr. Allen, however, mention the effect of tan in throwing down a copious precipitate from the serosity; but from the way in which it was obtained, by pressing it from serum that had been exposed to heat, we must conclude, that it still contained a portion of uncoagulated albumen, which would be acted upon by the tan. Thus it appears that the erroneous opinion, which these gentlemen derived, in the first instance, from the French chemists, was confirmed by the circumstance of their operating upon the substance in an impure state.

On the Nature of Oximuriatic Acid,

IN REPLY TO MR. JOHN DAVY. BY J. MURRAY, LECTURER ON CHEMISTRY, EDINBURGH.

From Nicholson's Journal, for April, 1811.

To Mr. NICHOLSON.

SIR, Edinburgh, March 11, 1811.

I SHALL beg your permission to occupy a few pages of your Journal with some remarks on the reply of Mr. J. Davy, in your last number, to the observations which I had offered on Mr. Davy's opinion of the nature of muriatic and oximuriatic acids.

The gentleman first remarks, that I have not attended to the distinction between theory and hypothesis, but have made promiscuous use of the two words, and have hence taken an incorrect view of his brother's opinion, which, he adds, is a theory, not an hypothesis.

The free signification generally given to the word theory in chemical language is so well understood, that I did not suppose it could have given rise to any ambiguity requiring to be pointed out. Theory, strictly understood, implies, no doubt, a principle established by just induction from individual facts, and applied to the explanation of phenomena; while in an hypothe-

sis a principle is assumed, whence phenomena are attempted to be explained. But there is an intermediate kind of reasoning or speculation, in which there is partly generalization, partly hypothetical assumption—a principle being professedly inferred by induction, but requiring the aid of hypothesis to apply it to all the phenomena connected with the subject. To this the terms opinion, theory, and hypothesis are often indiscriminately applied; and theory in chemical language has more frequently this signification than any other. The successive revolutions in chemistry would lead us to doubt indeed if it can be often justly used in its more strict sense, implying a perfect induction, the certainty of which subsequent discoveries cannot change. The theory of Stahl, as it has been named, which at one period commanded universal assent, was quickly subverted. Much of the theory of Lavoisier, ample and conclusive as the evidence appeared to be on which it is founded, must, if late speculations be just, share the same fate. And even those more partial inductions, which appeared to have the utmost certainty, are many of them, it now appears, doubtful. To a philosophic inquirer this may perhaps suggest some caution in applying the term theory in its strict signification, and it may guard him against the most common of all errors—an undue confidence in our speculations, and the belief that the opinions of our day are demonstrated truths.

Mr. J. Davy's mistake, (and which I should have obviated in my former paper, had I supposed any one attending particularly to the subject, and accustomed to scientific deduction, could have fallen into it, and had I not wished to avoid holding out unnecessarily Mr. H. Davy's opinion as purely hypothetical,) is the supposing this opinion to be a theory in the strict sense of the term. In stating the grounds of this opinion, he exposes too, very clearly, the source of his error. Mr. Davy, he remarks, " combines oximuriatic acid gas with hidrogen gas, and forms muriatic acid gas. In his theory, muriatic acid gas is a compound of oximuriatic gas and hidrogen. He combines oximuriatic gas with sulphur, phosphorus, and the metals; and in his theory the resulting substances are compounds of the inflammable and metallic bodies respectively, and oximu-

riatic gas. Here we perceive no supposition, but a simple expression of facts, and this I humbly conceive is pure and genuine theory." It is of main importance Mr. Davy also remarks, " that expression of facts be not misrepresented. It is of great consequence that things be not termed notions, that theory be not considered as speculation."

The proper expression of the above facts is, that from the mutual action of oximuriatic gas and hidrogen, muriatic acid gas is obtained, that from the mutual action of oximuriatic gas and inflammables, or metals, such and such substances are formed. That muriatic acid is a compound of oximuriatic gas with hidrogen, or that these substances are compounds of oximuriatic gas with the respective metals or inflammables, are inferences, which may be true, or may be false. They appear no doubt to be the most direct inferences, but the most obvious and direct conclusion from an experiment may not always be the just one. The shortest way of convincing Mr. J. Davy of this will be to state to him in the very form of expression, which he employs in the above quotation, conclusions which he cannot admit. I combine, I may say, oxide of mercury and muriatic acid, and form calomel. I conclude therefore, that calomel is a compound of muriatic acid and oxide of mercury. I combine muriatic acid and potash, and by dissipation of the water I obtain a solid product, which I consider as a compound of the muriatic acid and potash: and I perceive in these conclusions no supposition, but a simple expression of facts. Mr. Davy will in this however soon correct me, and inform me not merely that they are suppositions, but conclusions altogether false. He will perceive therefore, that the inference which appears most obvious and direct is not always just; that it is sometimes necessary to take a more extensive or deep view, and will perhaps be convinced, that he has erred in the notion he has formed of the kind of induction, which constitutes a genuine theory.

The cause of errors such as this is, and which has long been recognised as the fertile source of false speculation in physics, is the taking into view only part of the facts which belong to the subject—those which appear most favourable to

our induction, instead of taking the whole into consideration, and from this general view forming the most probable conclusion. If the facts above quoted from Mr. Davy were all that are connected with the question, his conclusions might appear to be just. But there are others equally connected with it, to explain which, various hypothetical assumptions must be made, the probability of which ought to be considered, and the whole compared with any other induction that may be formed.

Thus in distilling muriatic acid from black oxide of manganese, the oxide loses a portion of its oxigen, the muriatic acid disappears, and oximuriatic acid is obtained. The direct conclusion from this experiment (and it is equally direct with Mr. Davy's conclusion from the experiment of detonating oximuriatic gas and hidrogen) is, that the oxigen of the oxide has combined with the muriatic acid, and formed the oximuriatic; and to obviate this he is obliged to *suppose*, that the oxigen of the oxide combines with the hidrogen of the acid, forming water, and setting free the oximuriatic acid. If we expose liquid oximuriatic acid to solar light, oxigen is expelled, and muriatic acid remains; and the most direct inference from this is, that oximuriatic acid is a compound of muriatic acid and oxigen. Mr. Davy must have recourse to a less obvious explanation, and *suppose* a portion of water to be decomposed, its oxigen disengaged, and its hidrogen combined with the oximuriatic acid. And he has no proof of the formation and decomposition of water in these experiments, farther than that they must take place, if his theory be true.

If we consider therefore these two opinions under different aspects, if we set out as it were from different points, each will appear an induction, which to apply to all the phenomena farther requiries some hypothetical assumptions. Considering the experiment of the production of oximuriatic acid, by distilling muriatic acid from substances which impart oxigen, the most direct conclusion is, that it is a compound of oxigen and muriatic acid; but to apply this in explaining the agencies of both acids, it is necessary to suppose in some cases changes to occur: the formation or decomposition of water for example, of which we have no independent proof. Considering

on the other hand the experiment of the mutual action of oximuriatic and hidrogen gases, the most direct inference is, that muriatic acid is a compound of these substances; but in adopting this as the basis of a theory, it is equally necessary to advance suppositions, and in particular to suppose without any actual proof, that in different cases water is formed or decomposed. These suppositions are not always required in the two systems in relation to the same fact; but still, wherever an hypothesis is required in the one, a corresponding hypothesis will be found necessary in some part of the other. They occur on the whole just as frequently in Mr. Davy's system as in the opposite one, and they are as little supported by actual proof. He, for example, has no better proof, that water is formed when muriatic acid is distilled from black oxide of manganese, than I have of its formation in the mutual action of oximuriatic gas and hidrogen; or that it is decomposed when oximuriatic acid is exposed to solar light, than I have of its decomposition when a metal is acted on by muriatic acid gas. The two opinions are perfectly alike with regard to the evidence on which they rest derived from the above facts; and to select in one of them that part where the induction appears direct, and take it for granted that it is true, the hypothetical assumptions, which must be farther made, necessarily follow: but to pursue the reverse method with regard to the other, to represent it as an hypothesis, by bringing forward the parts which require the assumption of hypothesis, and neglect or reject the more direct induction, is a mere sophism. This is exactly what Mr. J. Davy does, no doubt without being aware of it, his error arising from the confined and partial view he has taken of the subject.

All this is so obvious, that some apology is due to your readers for having illustrated it at any length. This has however been rendered necessary by the tone this gentlemen has assumed through the whole of his observations. He has held out the opinion of his brother as a genuine theory resting on indubitable evidence, and has thought himself at liberty to represent the reasoning I have employed as mere speculation, and the explanations I have here given as uniformly hypothetical, and hav-

ing on this account no pretensions to be put in contrast with the others. He will now perceive, that the subject may be presented under a different light; and he will not perhaps again hazard the assertion, that his brother's "conclusions are not tainted by the slightest admixture of hypothesis." Lest he should, permit me to add one or two illustrations, which are besides not unconnected with the subject.

On adding to nitrate of mercury, muriate of soda, nitrate of soda and calomel are formed; chemists have therefore been accustomed to conclude by rules of evidence, which they thought sufficiently certain, that the nitric acid combines with the soda, and that the muriatic acid combines with the mercurial oxide, forming the calomel. According to Mr. Davy's opinion however, though the nitric acid unites with the soda, the muriatic acid does not unite with the oxide, but these substances decompose each other, the oxigen of the oxide unites with the hidrogen of the acid and forms water, and the calomel is a compound of oximuriatic acid and metallic quicksilver. Again, on adding muriatic acid to potash we form muriate of potash, and on exposing this product to heat, so as to obtain it dry, we have hitherto believed, apparently on very strict induction, that the water was expelled, and that the dry product is a compound of muriatic acid and potash. But in Mr. Davy's new system it is supposed, that the acid and the potash decompose each other, the oxigen of the one and the hidrogen of the other combine and form water, leaving a compound of oximuriatic acid and potassium. On dissolving this in water new changes occur, the water is decomposed, the potassium receives oxigen, the oximuriatic acid, hidrogen, and a compound of muriatic acid and potash is again formed. Who does not perceive in all this abundance of hypothetical assumptions? assumptions I have no hesitation in saying more gratuitous and more complicated, than any required in the opposite system, or perhaps in any other chemical speculation.

Mr. Davy's opinion then I regard as an hypothesis; the opinion I have maintained I have also distinctly admitted in my former paper to be an hypothesis: each rests on an apparently probable induction, and is capable of being applied with more

or less probability to the various phenomena. Placing them on this ground I considered and still consider the common doctrine as superior in simplicity, in requiring less strained and less complicated assumptions, in affording explanations of facts which the other does not explain, and in according with the general system of chemical theory; receiving therefore all the support which the evidence on which that theory rests can give, while the other is anomalous, and has the weight of that evidence against it. I proceed to offer a few observations on some of the more particular topics of this discussion.

Mr. Davy remarks, that when I speak of muriatic acid I do not mean the whole ponderable part of it; water I consider as necessary to its existence in its gaseous state, and that it is to the substance free from this water that the term muriatic acid is applied. This is my meaning. But when he proceeds to the interrogations, has Mr. M. examined it in its insulated state? has he described its properties? or has any chemist ever obtained it? And when from the negative given of course to these he concludes, that I must regard muriatic acid gas as a compound of an unknown basis and water, he raises or magnifies difficulties of little importance, or at least not peculiar to this investigation. When I speak of real sulphuric or nitric acid, I mean the acid free from water, though it may not have been obtained in that state; and in thus using language which is familiar to chemists I am not aware, that I introduce any novelty of hypothesis. The real muriatic acid I consider as an acid from having reason to conclude, that it exists in its solid compounds, neutralizing the bases with which it is in combination; in the same manner as I consider real sulphuric or nitric acid as an acid from knowing, that they exist in combinations free or nearly so from water, exerting a similar neutralizing power. Mr. Davy may if he pleases suppose sulphuric or nitric acid to be a compound of an unknown basis and water, and conclude, that the supposition of the existence of these acids free from water is "speculation in the strictest sense of the word." But no chemist will find any difficulty in this. The apparently greater influence of water on the chemical powers of muriatic acid than the other acids probably arises from the facts with regard

to the latter being more fully developed. On this subject I am at present engaged in an experimental investigation.

Mr. Davy proceeds to examine the objections which I had offered to his brother's opinion. To the remark, that there is some improbability in the hypothesis of two acidifying principles, he replies, that the only legitimate mode of reasoning is induction; and that we are not to judge of Mr. Davy's views by considering their probability or improbability *à priori*, but by a reference to facts. The observation would be just, were Mr. Davy's opinion a theory; but while it is an hypothesis the objection retains its force, for of two hypotheses proposed on any subject, it is fair in contrasting them to bring into view the probability *à priori* of the principle of the one compared with that of the other. The concession that he makes, that there is properly no acidifying principle, is rather more in point, and I have remarked in my Elements of Chemistry, in giving a view of the theory of acidity, that the supposition of acidity being a property rather derived from the base of acids than from their common principle is most conformable to Mr. Davy's speculations. Still the similarity of oximuriatic acid to oxigen so as to class them as acidifying principles is very remote; for allowing it to combine directly with any substance, it forms no acid in the common definition of the term, or any substance analogous to acids, but in its supposed combination with hidrogen, the subject of dispute; and the acidity of this, and its analogy to the common acids, are indeed such as to afford on the other hand as striking an anomaly in comparison with its other supposed combinations.

To the explanation I had given of the want of action between charcoal and oximuriatic acid, that it is owing partly to the absence of the water required in the constitution of muriatic acid gas, and partly to the absence of that disposing affinity exerted by the acid in establishing other combinations, Mr. Davy replies, that it would be less objectionable were it less hypothetical; that I do not prove muriatic and carbonic gases contain water, I only suppose it—To this mistake that the one opinion is a theory, the other an hypothesis, I have been forced to recur so often, that it is scarcely necessary to point it out.

Both are hypotheses, and if I can explain a fact by the principle of the one, which admits of no explanation by the principle of the other, the superiority of the former is with regard to this point sufficiently established. Besides, the application of the doctrine of disposing affinity alone affords an explanation of this fact. I consider it therefore as still giving a superiority to the common system, for nothing can appear more anomalous than that of all inflammable and metallic substances none but charcoal remains unchanged by oximuriatic acid, and nothing can be more satisfactory than to have a cause assigned for this. Mr. Davy considers it as of little importance, though it is this apparent anomaly, and the supposed difficulty of accounting for it according to the old opinion, which gave rise to the new doctrine, or at least first suggested the suspicion, that oximuriatic acid does not contain oxigen; he is satisfied with the observation, that we have no right to expect from a theory the explanation of ultimate facts; that is, not of comprehensive facts arrived at by a generalization which cannot be carried farther, for the fact in question is not of this kind, but of individual facts which the theory will not explain; a very safe conclusion, in which it may be proper for him to abide. This, he adds, is an ultimate fact, " one of those which constitute as it were the axioms of the science;" and he adds, that he is glad it is not tortured by hypothetical explanation. How it is elevated to an axiom, taking this word in its common sense of a fundamental proposition, self-evident, and therefore not admitting of demonstration, I do not understand; at least I do not perceive how this definition applies to the proposition, that oximuriatic acid will not combine with charcoal. Could Mr. Davy discover even a supposition by which this fact could be explained, I have little doubt that he would gladly remove it from the class of axioms, and from the facility with which he admits so many other suppositions, that he would submit it even to some degree of torture, to bend it into conformity with the system.

On the fact with regard to the influence of water in favouring the disengagement of carbonic acid from bases with which it is combined, it is scarcely necessary to make any observation.

I was aware, that it might operate by its affinity to the base, for I had suggested this some years ago, in the discussion of the question with regard to the influence of water on the constitution of elastic fluids, in my chemical system. But it farther appeared to me probable, that it operates likewise by its affinity to the acid, as there is reason I think to suspect, that the agency of water is important with regard to all acids. It is still however a case in point, since it is an example of the powerful agency of water in modifying affinities, and of a decomposition not being capable of being effected unless it be supplied; as well as a proof that a body, whether it be an earth or an acid, cannot be obtained insulated from its combinations, unless it receive the portion of water which it requires.

The argument, that from the strong affinity of muriatic acid to water a portion of it will be retained in combination with it in the elastic state, I hold valid, notwithstanding the reply, that muriatic acid does not exist as a gas in combination with different proportions of water, " for the proportion of hidrogen gas produced from muriatic acid gas, acted on by different metals, is always the same, whether it has been exposed to the influence of drying salts or not." The inference which would render this reply conclusive does not follow; admitting that muriatic acid gas cannot exist with different proportions of water, the only inference then would be, that muriatic acid gas obtained by the usual methods is in as dry a state as we can obtain it insulated. But I doubt altogether the alleged fact; for Dr. Henry found by the more unexceptionable mode of the action of electricity, that recent muriatic acid gas affords $\frac{1}{14}$ of its bulk of hidrogen, while after the full action of muriate of lime it affords only $\frac{1}{35}$ of its bulk. The inference in the reasoning on this subject, from the affinity of potassium to oxigen, arises from a misconception of the theory of Berthollet, on which the original argument is founded, so obvious that it does not require to be pointed out.

Mr. Davy proceeds to some observations on my experiments, on which I shall offer few remarks, as I shall probably have an opportunity of engaging in the discussion of this part of the subject, to greater advantage, when better acquainted with the

experiments brought forward in opposition to those I have stated. I may be allowed to say, that of the accuracy of the results I obtained, particularly of those which appear to be questioned, the formation of carbonic acid, when carbonic oxide, hidrogen, and oximuriatic acid gases are submitted to mutual action, I am fully convinced. I had preserved the notes of these experiments, some of them written by Mr. Ellis, others by myself, at the time they were made; and in all of them carbonic acid was formed, though there often remained a sensible quantity of carbonic oxide, of which I have taken notice in my paper. How Mr. Davy infers, that the whole carbonic oxide should be converted into carbonic acid, in order to admit the inference, that oxigen is communicated from the oximuriatic acid, I do not comprehend. It is sufficient, if a portion of carbonic acid is produced. No notice perhaps is due to the remark, that in one of my experiments a small residue of common air was observed, whence it is inferred, that the conversion of carbonic oxide into carbonic acid might be owing to the presence of atmospheric air. Carbonic oxide gas requires for its conversion into carbonic acid by detonation with atmospheric air a quantity equal at least to 2½ of its bulk; and not a twentieth of this quantity could have been introduced even in the most inaccurate mode, in which Mr. J. Davy may suppose the experiment to have been performed. The small portion observed had originated from the minute quantity disengaged by the force of the detonation from between the sides of the tube and the quicksilver, which, added to what might be disengaged by the transmission through the water, and agitation with it after the explosion, and to the slight admixture to which all elastic fluids are liable, (expressed in one of Mr. Davy's own experiments, by the phrase of "no more impurity than might be expected in the air in the gases"), had formed a quantity capable of being discovered. It could obviously detract nothing from the conclusiveness of the experiment, independent of the circumstance, that it was not observed in the more decisive experiment of the slow mutual action of these gases.

He is willing to admit the accuracy of my experiments, so far as they relate to the want of action between carbonic oxide

and oximuriatic acid, when water is excluded. This is a fact, which, though it presents an anomaly in the new hypothesis, is not absolutely hostile to it; and I have, it seems, "very satisfactorily proved" this nonaction, and that without the agency of water no carbonic or muriatic gas is formed. But my experiments, which afford results presenting a difficulty in the theory, are it seems inaccurate; and beside endeavouring to make this apparent by various observations, Mr. Davy informs us, that either alone, or in conjunction with his brother, he has, if not repeated them, (which would have been the most direct mode of proving their inaccuracy), at least made experiments of a similar kind, and with very different results. Thus he has performed some of the experiments of Cruickshank on the carburetted hidrogen gases. The source of fallacy, which I supposed would have been contended for, in the presence of a portion of oxigen in the composition of these gases, he seems to think of little importance; the principal source of fallacy is supposed to be in the presence of water, and he therefore made the detonations over mercury, and "never obtained carbonic acid gas, though oximuriatic gas in great excess was employed." I shall not say any thing of the source of fallacy, which I conceive has given rise to this observation, until I have repeated the experiment with the necessary precautions. I shall only observe, that Mr. Davy does not directly deny, that carbonic acid is formed, he has only performed the experiment without obtaining it, and he does not inform his readers what were the actual results. The hidrogen must of course have been removed by the action of the oximuriatic acid: What became of the carbon? Was it precipitated in the state of charcoal? Or did it remain in the state of carbonic oxide? Or did the gases form a ternary combination? and how was the absence of carbonic acid established? Yet with all these deficiencies a result thus generally stated is brought forward in opposition to experiments minutely detailed. Neither does he deny the formation of carbonic acid in my experiment of submitting carbonic oxide, hidrogen and oximuriatic gases to mutual action: he supposes its formation owing to the presence of atmospheric air, as I have already stated, or to the introduction of water after the

experiment; the gratuitous assumption being made of the formation of a triple compound of carbonic oxide, hidrogen, and oximuriatic acid, by which this water would be decomposed. Hence, in the experiment made in conjunction with his brother, ammonia was used instead of water to absorb the muriatic acid, and it is inferred, that no carbonic acid was formed. It is singular however, that no attempt appears to have been made to discover this acid, its nonformation is inferred from the gas which remained after exposure to water burning with the same coloured flame as carbonic oxide, whence it is concluded to have been this gas, with the intermixture of nitrogen from the ammonia, its volume being admitted to be different from that of the carbonic oxide employed; and this negative result, open to such obvious fallacies, is placed in opposition to the positive production of carbonic acid in my experiments. Lastly, after all these experiments made without carbonic acid being obtained, and after the labour bestowed in endeavouring to prove experiments in which it was produced inaccurate, or to account otherwise for their results, a gas is discovered (the discovery of it is mentioned in a note, it having been made after Mr. J. Davy's paper was written) which it is admitted is capable of converting carbonic oxide into carbonic acid, and which is procured from the same materials as oximuriatic acid, and by a process apparently not much different from that which is usually followed.

There remains one point to which it is necessary to give some consideration. Mr. Davy gives a reason why, as he supposes, the presence of hidrogen cannot, even on the hypothesis I maintain, favour the conversion of carbonic oxide into carbonic acid by oximuriatic acid. In submitting these three gases to mutual action, the hidrogen, in combining with oxigen from the oximuriatic, can form no more water than is required to the constitution of that portion of muriatic acid with which this oxigen had been combined, and hence there is no superfluous water to be afforded to that portion of muriatic acid, which remains to be formed in the oxigenation of the carbonic oxide by the remaining oximuriatic acid. "It is impossible therefore," says Mr. J. Davy (in the style which he permits

himself to use) " that the latter portion of muriatic acid can become gaseous;—yet it does become, according to the experiment, muriatic acid gas, which is a contradiction, and by itself a proof of the inaccuracy of the hypothesis."

This objection had occurred to me, and I had stated it as well as the solution of it to some of my friends. Of course however, a difficulty of this nature could not be urged against the fact, that the conversion of carbonic oxide into carbonic acid, which does not happen when oximuriatic gas alone is added to the former gas, takes place when there is an addition of hidrogen. And as farther the difficulty appeared to me capable of being solved, I thought it unnecessary to add to the length of my paper by taking notice of it. It is explained, I conceive, in the following manner. Muriatic acid gas may be formed with a less proportion of water than is necessary to its full saturation; the proof of its existence with an inferior proportion of water to what it contains at its first disengagement in the usual process for obtaining it I have already stated, and there is nothing improbable in the supposition, that it may exist gaseous with even less water than can be inferred from that proof. And in all elastic fluids small portions of water I believe exist, of which it is probably impossible to deprive them entirely. When a mixture, therefore, of carbonic oxide, hidrogen, and oximuriatic acid gases is submitted to mutual action, the hidrogen attracting oxigen from the oximuriatic acid, and forming with it a portion of water, this water may favour the production of a larger quantity of muriatic acid, than the quantity merely with which that oxigen had been combined, if any powerful affinity is at the same time exerted, having a tendency to produce that acid: now such an affinity is actually exerted by the carbonic oxide, hence it is enabled to operate with effect, an additional portion of oximuriatic acid is decomposed, and by the communication of oxigen, portions of muriatic and carbonic acids are formed. This is farther aided by the portion of water which must have been contained in the portions of hidrogen, carbonic oxide, and oximuriatic gases, suffering these changes, even though they had been previously rendered as dry as possible. Of itself, this would not be sufficient to favour

the decomposition of the oximuriatic acid, but added to the quantity formed by the oxigenation of the hidrogen it may be adequate to this effect, to the extent I have stated. The operation of this small quantity of water, remaining in elastic fluids in the driest state to which they can be brought, is very well shown in the results of the detonation of hidrogen and oximuriatic gases, as related by Mr. H. Davy. Mr. J. Davy, indeed tells us, that he has seen this experiment made a number of times, " and though correctly made, there is no condensation attending their union." But his brother informs us, that, when the gases are mixed over water, introduced into an exhausted vessel, and fired by the electric spark, there is a condensation of from $\frac{1}{10}$ to $\frac{1}{20}$ of the volume. I have attempted, he continues, " to make the experiment in a manner still more refined, by drying the oximuriatic acid, and the hidrogen, by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures, but I have never been able to avoid a slight condensation, though in proportion as the gases were free from oxigen or water, this condensation diminished." This condensation must be owing to the water remaining in the gases even after the action of muriate of lime; and it is this, no doubt, which partly operates in the experiments I have related, and favours the result.

Without any reference to this experiment I would farther observe, that the conversion of carbonic oxide into carbonic acid, when oximuriatic acid acts upon it with the admission of a little water, is conclusive against Mr. Davy's hypothesis. The only mode in which the result can be accounted for, in conformity to that hypothesis, is to suppose the water to be decomposed. This I had anticipated, and had observed, that of this supposition we have not only no proof, but we have sufficient reason from known facts to reject it. " Water is not decomposed by oximuriatic acid gas, or by carbonic oxide gas; there is no reason to conclude, that its decomposition can be effected by their action, when they are presented to it merely in a mixed state; and the more obvious operation may be regarded as the real one, that it acts by its affinity to muriatic acid."

In answer to this, Mr. J. Davy remarks, have we not the most indubitable evidences of the decomposition of water by oximuriatic gas? " Pass this gas and steam together through a tube heated to redness, oxigen gas will be produced, and muriatic acid gas formed. Detonate this gas and hidrogen together, and a similar formation of muriatic gas will take place. With these facts in view, who can hesitate in asserting, that water is decomposed by oximuriatic gas, that its hidrogen is attracted by this substance, and its oxigen consequently set free."

The decomposition of oximuriatic gas by detonation with hidrogen gas, or its combination with that gas, according to Mr. Davy's view, affords no proof of the power of water to decompose it. Admitting the decomposition of oximuriatic gas by water at ignition, and supposing that we know nothing as to the mutual action of these substances at low temperatures, the conclusion would not follow that at such temperatures this decomposition would take place. But to draw this conclusion in the very face of the fact itself is, I confess, a mode of reasoning altogether new to me. We know, that oximuriatic acid is not decomposed by water at natural temperatures; and knowing this I have no hesitation in asserting, notwithstanding Mr. Davy's indubitable evidences, that the supposition of water being decomposed in the slow mutual action of carbonic oxide and oximuriatic gases is altogether gratuitous, and made to avoid a difficulty, which cannot otherwise be explained.

And lastly let it be remarked, that putting aside all these experiments the two hypotheses still remain at least on equal grounds. The experiments were supposed to afford evidence in favour of the common doctrine of the nature of the relation between muriatic and oximuriatic acids; and were they proved to be inconclusive, it would only stand in the same rank with the opinion advanced by Mr. Davy; it can be applied, as I trust I have shown in my former paper, to the explanation of all the phenomena, and to some of them with more advantage than the other; and it has besides, as an hypothesis, that superiority from general considerations, which I have also stated to belong to it. I am, with much respect, your most obedient servant,

JOHN MURRAY.

A Case of Rupture of the Uterus.

From the Medical and Physical Journal for March, 1808.

Mrs. Skinner, the wife of a farmer in this neighbourhood, aged 39 years, is of a strong and healthy constitution, but was lately much weakened by a severe peripneumony. Previously to the confinement which has afforded the subject of this case, I have attended her in four labours. They were all difficult in consequence of the small dimensions of the pelvis, and in two of them, where the children were very large, and the bones of the cranium much ossified, I was obliged, on account of the dangerous state of the mother, to have recourse to the crotchet; as the most violent action of the uterus, for three and four days, was unable to bring the head so low into the pelvis as to admit the application of any other instrument, and her recovery was each time very slow and difficult. In the two other labours, the children being small, and the cranium less ossified, the delivery was effected by the action of the uterus.

She was taken in labour for the fifth time, at the full period of gestation, early on the morning of September 30 last. On my arrival, I found the os uteri fully dilated, the external parts wholly relaxed, the head of the child resting on the pubes, the membranes entire, and the pains frequent and very strong, accompanied with a small discharge of blood. During one of those pains, the membranes were protruded through the os externum, and in that state the waters were discharged. The head now gradually advanced, and, in about three hours, became forced into the narrow part of the pelvis. When it had arrived in this situation, though during every pain, it still appeared slowly to descend, she complained of a more continued and severe pain in the lower part of the uterus, with so great a degree of restlessness, that she could scarcely be prevailed on to remain in bed. As her disposition is naturally passive and accommodating, I particularly noticed this circumstance; and for the purpose of moderating the action of the uterus, I administered, without delay, a full dose of tinct. opii. The

pains, within a quarter of an hour after this, had *entirely* ceased, which I then attributed to the effect of the opiate: still, however, the restlessness was not lessened, and it was accompanied with great anxiety.

She soon afterwards called me to her bed-side, and requested me to apply my hand on the abdomen; as she expressed herself, that during the *last pain*, "something had slipped out "of its place." I perceived an irregular tumor, about the size of a man's fist on the lower part of the abdomen towards the right side. I could not distinctly feel that it contained any *particular* part of the child; but the circumstance, connected with the other symptoms, impressed me with the belief, that the uterus had ruptured at this part. The light of the morning enabled me also to observe her countenance. She was pale, breathed laboriously, and was apparently sinking. A violent vomiting supervened, the pulse was very quick and feeble, and the extremities covered with cold perspiration.

As I considered that nothing but speedy delivery could afford her the smallest chance of recovery, from a situation which was almost hopeless, I proceeded, as soon as I could obtain the proper instruments, to open the cranium, which still preserved its situation in the pelvis. But on the application of the perforator, before I could use a sufficient degree of force even to penetrate the scalp, it suddenly receded into the cavity of the uterus. I immediately followed it with my hand. The uterus was so relaxed and yielding, that I was enabled, with the greatest ease, to bring down both feet into the vagina.

It was not to be expected that the child could be extracted through a narrow pelvis in so short a time as to preserve its life, and, indeed, it was this consideration, which at first determined me on the use of the *crotchet*, in preference to turning; the application of any other instrument being, from the situation of the head, impracticable. The child proved to be so large, that it was with great difficulty I could bring the *thorax* through the pelvis, and it was not till I had applied the blunt hook over one of the shoulders, that I could bring the body so low as to reach the arms: but I was unable to deliver the head, till I had lessened its bulk, and assisted by fixing the crotchet on the in-

side. As it was in vain to wait for the natural expulsion of the placenta, I soon after the delivery, proceeded to introduce my hand again into the uterus.

If any doubt had before remained of the nature of this case, it was now wholly removed, as I passed my hand without difficulty, through the aperture into the abdomen. I applied my fingers on the smooth *internal* furface of that cavity, when the gentleman who assisted, and myself, were both satisfied, by placing our hands, at the same time, on the *outside*, that nothing but the thin parietes of the abdomen intervened. The ruptured part, as nearly as I could ascertain, was in the anterior part of the cervix uteri, extending also into the vagina. There was fortunately none of the intestines in the way, nor did I think myself justified in searching for them, but hastened to withdraw my hand into the uterus, when I removed the placenta, and resigned, as I had then too much reason to fear, my unhappy patient to her fate.

No extraordinary discharge followed the delivery. She continued during the remaining part of this day in a very languid state, with violent and frequent vomiting.—On the following morning, having had some sleep during the night, she appeared considerably revived. The pulse was 120 in a minute, and stronger. There was no general enlargement, nor pain in the abdomen, except on pressure, when the uterus, was described to be exquisitely tender, and was felt to be contracted about the size of a child's head. She had voided urine without difficulty, but there had been very little discharge of lochia, and scarcely any thing had remained on the stomach. In this state, she continued with little variation for the first four days. —The regimen during this time was strictly antiphlogistic, and the medicines were saline draughts, with gentle aperients, and an opiate at night.

From the 5th to the 12th day, she became gradually into a state of the most distressing restlessness and anxiety: constantly endeavouring, but unable, to change her posture. She repeatedly vomited a *dark coloured* fluid, and had frequent motions of a similar appearance. The pulse was very irritable and weak, and varying from 130 to 150 in a minute; the abdo-

men was enlarged and painful; the uterus extremely tender; the lochia serous, and in very small quantity.—The Peruvian bark in different preparations was now directed to be taken freely, and light nutriment, in all the forms that could be devised, was prescribed for her. But almost every thing was ejected from the stomach, and except the opiate at night, she could not, at last, be prevailed on to take any medicine, and with great difficulty, food. A few hours' sleep, procured by the opiate, was the only relief she experienced.

Such was her situation on the 12th day, at which time the size and tension of the abdomen indicated an extravasation of fluid into that cavity. As no medicine could be retained on the stomach, the ungt. hydrargyri fort. was directed to be rubbed into the abdomen twice a day, with a view, if possible, to produce its absorption.

On the following morning, the enlargement was much subsided, in consequence of her having had several *very profuse* motions during the night. The uterus was become less tender; the pulse reduced to 100, and the restlessness, anxiety, and every other unfavourable symptom, abated. From this day, she continued gradually to amend, but whether this abatement of the unfavourable symptoms was occasioned by the mercurial action, or whether the application had any effect in producing the extraordinary discharge from the bowels, is uncertain. There appears, however, to have been scarcely time, nor does it seem that the quantity used was sufficient for so powerful an operation.—Yet though I was more disposed to attribute it to a critical effort of nature, than to an effect of the medicine employed, I considered it, at least, as an inducement to continue its use: and the amendment taking place so soon after its first application, my patient was very solicitous to persevere in it. In eight or ten days, a soreness of the gums obliged me to desist. During this period, a larger discharge of lochia came on; the stomach by degrees retained food, and the more alarming symptoms had so far subsided, that I could not but indulge the most pleasing expectation of her recovery.

About three weeks after delivery, she was attacked by the peculiar swelling of the leg and thigh incident to lying-in women, (Phlegmatia Dolens.) This considerably retarded the

progress of her recovery, and did not wholly subside for another fortnight.

The debility occasioned by so long and painful a confinement, may be supposed to have been very great, and it was not till five weeks after delivery there could be said to be any return of strength. She was enabled then to be taken out of bed for an hour daily, and her appetite began to increase. The swelling of the abdomen was entirely reduced, but there was still felt, on pressure, a tenderness in the region of the uterus. She also complained of a very troublesome sensation near the spine of the left ilium, when in an erect posture, or when lying on the opposite side, which she described "as if something was drawing away from that part:" probably, the consequence of an adhesion formed there, during the period of peritoneal inflammation. She went on rapidly, from this time, in a state of convalescence, and is now so well recovered, as, except a diminution of strength, to feel little inconvenience from the accident.

The above is an addition to the few instances of recovery, with which we are acquainted, from the most formidable accident that can occur in the practice of midwifery; and though there may be but little practical information to be derived from it, the recording of it must tend to inspire us with a greater degree of confidence in the powers of nature, even in the most desperate cases, where the impediments to the exertion of those powers are removed.—The only two instances of ruptured uterus which have fallen under my own observation have been in women with narrow pelves, and in both of them it took place in or near the cervix uteri. A degree of weakness in any part of that organ, disproportioned to the resistance to be overcome, must always predispose it to rupture. But if the weakness of structure should exist principally near its *cervix*, that part, *after the evacuation of the waters*, is in danger of becoming violently compressed between the head of the child and the edge or some projecting part of the pelvis; and in such a situation, the probability of its giving way must be very great. Whenever, therefore, delivery is protracted on account of an extraordinary degree of resistance, either from the smallness of the pelvis,

or a rigidity of the soft parts, the practitioner has been much cautioned to avoid the premature rupturing of the membranes. And if, *after that has taken place*, the woman should complain of an unusually severe pain, confined to a particular *part* of the uterus, distinct from, more agonizing, and sometimes more constant than the labour pains, symptoms which indicate the too forcible compression of some *portion* of the uterus, he cannot be too much upon his guard, nor too prompt in endeavouring to moderate its action, and to lessen the resistance it has to overcome.

Both these purposes, if the difficulty should depend solely on a *rigidity of the parts to be dilated*, may, as far as it is in our power, be accomplished by large doses of opium, with bloodletting in proportion to the strength of the patient. But, as is more frequently the case, if the resistance should be occasioned by the *narrowness of the pelvis*, it becomes a subject of consideration, whether the magnitude of the danger we apprehend and wish to avert, will not justify us in effecting the delivery without further delay: by turning, if it be thought compatible with the safety of the mother and the child, or otherwise even by lessening the bulk of the head, should it be situated too high to admit of our applying the forceps or the vectis.

When, notwithstanding all our precautions, a rupture of the uterus has unhappily taken place, the necessity of a speedy delivery is, I believe, generally considered as unquestionable.—Every additional case of recovery, under these circumstances, should impress us the more strongly with the propriety of the practice, and deter us, under an idea of the unavoidable fatality of the accident, as has been sometimes the case, from allowing the woman to remain undelivered, which can afford the least possible chance to the mother, and must *necessarily* prove the destruction of the child. I am, &c.

THOMAS HUGO.

December 24, 1807.

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THOMAS HUGO.

December 24, 1807.

A Case of Intus-Susceptio,

WITH REMARKS, BY THOMAS BLIZARD, ESQ. F. R. S. AND R. S. EDINBURGH, AND SURGEON TO THE LONDON HOSPITAL.

From the Medico-Chirurgical Transactions, Vol. I.

HAVING lately had an opportunity of examining the body of a male child, aged five months, who died in consequence of an intus-susceptio, I take the liberty of transmitting an account of the appearances, with a drawing by Mr. Clift, to the Medical and Chirurgical Society.

Intus-susceptions, to a small extent, are not unfrequently noticed, particularly in children, without any mark of having produced obstruction or inflammation of the alimentary canal; but, as a disease, it, happily, is of rare occurrence. The subject of the present case had, previously to the attack, been always healthy, and free from bowel-complaints. On Sunday, the 28th of February, he was seized with vomiting, accompanied with constipation, and other signs of disordered functions of the bowels. There were, at first, some small discharges of mucus by the anus; but, after Monday, the discharges, which were frequent, were principally of blood. The abdomen was tense, and on the left side, there appeared a tumor, about the size of an egg. A hiccoughing commenced on the Tuesday, and continued until death, which took place on Thursday evening, the fifth day from the accession of complaint.

On dissection it appeared that the tumor on the left side was produced by an intus-susception: about six inches of the intestinum ileum, the cœcum, with its appendix, the ascending colon, and transverse flexure, being contained in the sigmoid flexure of the colon, extending into the rectum. The intus-suscepted parts were in a state of complete strangulation, and perfectly black. The lower part of the ileum (about ten or twelve inches) immediately above the intus-susception was a little inflamed; but, otherwise, the effects of this derangement of parts were so strictly confined to the intus-suscepted bowel, that had the child's constitution been able to sustain its separation, the inflammation necessarily accompanying this process would, no doubt, have produced an union of the ileum with the lower

part of the colon; the continuity of the canal would thus have been maintained, the separated part might have passed, and the child have recovered.

I feel justified in hazarding the opinion of the possibility of the child's doing well under these circumstances from the relation by Dr. Baillie, in the second volume of the transactions of a society for the improvement of medical and chirurgical knowledge, of two cases in which gut was passed per anum.

The one case is that of a lady, about fifty years of age, who, after much suffering with violent pain of the stomach and bowels, more especially on the left side, accompanied with vomiting and constipation, about three weeks before her death, voided above a yard in length of intestine, which proved to be a portion of the colon. It is worthy of remark, that the pain was more especially seated on the left side, and that the evacuations, for many days, consisted merely of blood, and at that time were very numerous. In these respects there is a striking resemblance between this case and the child's just related.

Of the other case there are no further particulars stated, than that the person lived two years after discharging a portion of gut, about six inches long, and which proved also to be a part of the colon.

The learned doctor not having had an opportunity of investigating after death the processes of nature under these extraordinary circumstances, has proposed an ingenious explanation of the cases by supposing a membrane, formed of coagulable lymph, to have been produced round the mortified portions of intestine, by which a continuity of the canal was maintained. It is with the greatest deference I presume to offer a sentiment contrary to an authority so generally, and justly, respected; but is it not highly probable that these were cases of intus-susceptions, in which the intus-suscepted portions had been strangulated, had died, and were separated?*

* I am authorized by Dr. Baillie to say, that for a considerable time past, he has entertained the same ideas on the subject, which I have here stated.

I was not aware, when I wrote this paper, of Dr. Hull's having some years ago, (in the Medical Journal for 1802,) adopted a similar view of the point in question; and of Dr. Baillie's having then, with the most honourable candor, addmitted that he was probably mistaken in the reasoning which he had em

It is not very easily to be conceived, how, in the natural state of parts, the peritoneum, which binds down the cœcum and ascending colon, admitted of their being suddenly removed, as in the present case, to the left side into the sigmoid flexure of the colon: it is therefore very probable, that there was some variety in the mode of application of the peritoneum here, and which, if similar to what I once, some years ago, observed in a subject on which I was demonstrating, would readily permit such a displacement and might strongly predispose to it. The peritoneum was applied in a loose form to the cœcum and ascending colon, being, as it were, a continuation of the mesentery: by which these parts were, in some degree, left loose, and might easily pass over to the left side.

ployed with regard to it. I am happy in noticing this coincidence of opinion between Dr. Hull and myself, and in furnishing a case which tends so much to confirm its accuracy.

SELECTED REVIEWS.

Practical Observations on the Formation of an Artificial Pupil, in several deranged States of the Eye; to which are annexed, Remarks on the Extraction of Soft Cataracts, and those of the Membraneous Kind, through a Puncture in the Cornea, illustrated by Plates. By BENJAMIN GIBSON, Vice-President of the Literary and Philosophical Society of Manchester, and Surgeon to the Manchester Infirmary. London, 1811. pp. 154.

From the New Medical and Physical Journal for June 1811.

THAT, in cases of imperfect vision, the sight of a patient might be improved, and those of total interruption of sight might be restored, by forming an *artificial pupil*, has long been known; for Cheselden performed the operation with success, and his operation has been repeated by several surgeons since his time. The mode of forming the artificial opening has also been varied, and sometimes success has attended the operation; and at other times the attempt has failed, for want of a due discrimination of the causes producing the defect, and of the actual state of the parts concerned in the deranged state of the eye. Our author has entered into a minute examination of these circumstances, and has so clearly explained the actual states of the various parts of the eye in the different cases of disease, so ably described the mode of relief adapted to each, and has so happily illustrated his opinions by his successful practice, that we do not hesitate recommending a perusal of his work to all who are concerned in the treatment of diseases of the eye, which unhappily have become sufficiently prevalent in this country, from the time the British troops so gallantly reaped unfading laurels on the contested plains of Egypt.

There are several states to which the eye may be reduced by disease or accident, which can only be relieved by the formation of an artificial pupil. An opake state of the cornea, however, is by far the most frequent; and this may be complicated with disorganization and derangement of the internal parts of

the eye. It is chiefly in those cases, where the opacity occupies the centre of the cornea, leaving it more or less transparent towards its circumference *only*, that light is prevented from passing through the pupil, and vision so far impaired, as to require the assistance of art. When sight is lost from such a cause, it may be restored in a great measure, by the removal of a portion of the iris, opposite to the transparent part of the cornea, so as to form what has been termed an artificial pupil.

The varieties of derangement requiring an artificial opening, and in which he has operated, are stated by the author to be the following. Opacity in the centre of the cornea; central opacity of the cornea, attended with adhesions between the cornea and iris, which include only a portion of the inner border of the iris; central opacity of the cornea, attended with extensive adhesions to the iris, which include its whole border, or nearly so; central opacity of the cornea, and total opacity of the crystalline lens, or its capsule, with or without adhesions of the iris to the cornea; and, total closure, or obliteration of the pupil, attended with entire transparency of the cornea.

"The mode of operation for forming an artificial pupil, it is obvious, must be varied according to the circumstances of the case. When there exists merely an opacity in the centre of the cornea, without derangement in the internal parts of the eye; or when the pupil is closed after the extraction or depression of a cataract; in these cases the operation will be the most simple. But if, along with central opacity of the cornea, the iris have formed adhesions to its inner surface, with or without a corresponding diminution of the capacity of the anterior chamber of the aqueous humor, the operation becomes more complex. If again, along with opacity and adhesion of the iris, the crystalline lens or its capsule be opake, the adoption of stiii more complex expedients is necessary. The same may be observed of those cases, in which the pupil is totally obliterated or nearly so, and the iris adheres to the capsule of the crystalline lens; for this rarely happens without an opacity of the lens or its capsule, or of both. Sometimes however, a blow upon the sclerotic coat, particularly affecting the crystalline lens, and probably rupturing the edge of its capsule, is followed by the absorption of the lens, and leaves only the opake capsule."

The instruments employed by Mr. Gibson, are, in addition to the cornea-knife and curved scissars, recommended by Mr. Ware,

"1st. The very small hook, which is generally found in a case of extracting instruments.

"2dly. A small pair of forceps, which shut by a spring, and require the pressure of the finger to open them. Thus constructed, they are not affected by the pressure of the lips of a small incision in the cornea, through which they may be introduced; and when once a fragment is laid hold of, within the eye, the pressure of the spring will retain it between the blades, without the assistance of the fingers. One of the handles of the forceps is of considerable length, and the other short. By this means the forceps can be held steadily between the fingers and thumb, whilst the fore-finger is applied to the shorter handle, to open them occasionally.

"3dly. A small pair of iris-scissars, with one blade blunted at the point. They have handles like the forceps, but differ from them in this respect, that they open by means of a spring. These scissars are sufficiently minute to enter a small incision in the cornea, and to clip off portions of the iris, or of membraneous cataracts."

According to our author's experience, the permanency of the artificial pupil depends principally upon the size of the opening and healthy state of the iris and contiguous parts of the eye at the time of the operation. When the artificial pupil has been made almost as arge as the medium size of the natural one, and especially when the part of the iris removed has included its border, there appears no disposition in the opening to close. When, however, a mere narrow slip has been removed, when the iris, from previous inflammation, has become more vascular, than natural, or when it is complicated with adhesions to the capsule of the crystalline lens, in such cases its closure has occasionally taken place.

It does not appear that after the forming an artificial pupil the fibres of the iris possess any power of motion, so as to alter the size of the new opening. Mr. Gibson, when circumstances allow it, always prefers forming the artificial pupil to-

wards the external angle of the eye, and thinks that when the pupil is formed towards the internal angle, the sight is less extensive, the nose in some measure appearing to curtail the sphere of vision.

The following history will show Mr. Gibson's mode of operating, and the success attending it; it is not a partial selection he has here given us; for similar success, he assures us, has been almost uniformly the result of every operation of this kind, where the previous state of the eye afforded much expectation of deriving advantage from the operation.

"William Crooke applied at the Manchester Infirmary in May 1804, on account of blindness, which had existed for several years. On inspection, the pupil of each eye was found entirely closed, so that the state of the crystalline lens, or its capsule, could not be discerned in either. As, however, his loss of vision was occasioned by violent internal inflammation of the eyes, it was concluded that adhesions had formed between the opake capsule of the lens and the posterior surface of the iris, and that the crystalline lens was most probably in an opake state; as had uniformly been observed in a variety of similar cases.

"The operation, in this case, consisted of two stages. In the first, the substance of the crystalline lens was broken down in each eye. In both instances, it was soft, and readily yielded to the pressure of Mr. Hey's couching needle. The right eye was irritated, and some inflammation followed, which required the use of leeches, purgatives, and cooling collyria.

"The second part of the operation was performed in about a fortnight, when the eyes had perfectly recovered from the inflammation produced by the couching needle. It consisted in entering the knife through the cornea, into the anterior chamber of the aqueous humor, at the usual part. The point of the instrument was next passed through the iris, at the distance of about one third of its diameter from its root, so as to form an opening, whose length was also about one third the diameter of the iris, as nearly as could be calculated. The cornea-knife was then gently withdrawn, and the iris-scissars were introduced shut, until they reached the new formed opening in the

iris. They were then opened, and one blade was passed through the aperture formed in the iris, and the other blade directed anterior to that membrane, until the middle third of the iris was included between them. An incision was now carried through the included portion, commencing at the upper part of the opening made by the cornea-knife, and directed a little obliquely downwards; so that when a similar incision was made from the inferior part of the same opening, the pupil formed an irregular aperture, resembling in some degree an equilateral triangle.

"As soon as this portion of the iris was cut out, the opake fragments of the crystalline lens presented themselves, in a pulpy state, and were removed through the new pupil by means of the curette, until an inconsiderable portion only remained. The aqueous humor being in great plenty, the remaining fragments of the lens disappeared in the course of a week, and no further operation was necessary upon this eye.

"The right eye, which was not operated upon with the iris-scissars, until the left eye had recovered, required the scissars to be used twice, with some interval; principally, because the new formed pupil had contracted into too small bounds, and was not, therefore, similar to the pupil of the left eye. The opake fragments of the lens, also, had not a sufficiently free egress from behind, into the anterior chamber of the aqueous humor. By a second operation with the iris-scissars, the pupil was enlarged, and the whole of the opake fragments were removed by the curette. Some degree of inflammation again attacked this eye; which, however, is a rare occurrence after this operation. This patient returned home, and was able to follow his occupation as a weaver."

The author then proceeds to detail the circumstances under which little benefit is to be expected from this operation, and wherein he would not interfere, but at the earnest request of the patient. No uncommon effect of the Egyptian ophthalmia is to leave an opacity of the cornea through its whole extent, except an inconsiderable portion at its circumference, not exceeding in length or breadth one quarter the diameter of the cornea, which remains tolerably transparent. The opake por-

tion is generally thicker than common, and irregular upon its surface; in the majority of such cases, the means adopted induce a degree of inflammation, which the diseased structure of the cornea cannot withstand, and consequently such a state of the parts is unfavourable to the operation. However anxious the patient may have been for the operation, or however plainly its danger may have been pointed out to him, he never fails, Mr. G. remarks, to regret the loss of a scanty perception of light, which he enjoyed before it was undertaken.

The remainder of the volume is occupied by observations on the mode of extracting the soft and membraneous cataracts through a puncture in the cornea. These are highly interesting by their novelty and importance, and we must refer to the work itself, such as are desirous to peruse them. The following case of membraneous cataract illustrates Mr. Gibson's method of procedure, and may be taken as a fair estimate of his usual success in this operation.

"Millicent Smith applied at the Manchester Infirmary, on account of a considerable defect in the sight of one eye, the other having been destroyed some years before. On examination, an opake speck was observed in the centre of the capsule of the crystalline lens, surrounded by a slighter degree of opacity, of small extent. This state of the capsule occasioned a greater defect in vision, than might have been expected, especially as the eye appeared quite healthy in other respects. Although the crystalline lens, in this case, was most probably free from opacity, yet its removal was necessary; because the speck and opake portion of its capsule could not have been removed, without producing opacity in it.

"The operation, therefore, consisted in first attempting to depress the lens; this, however, proving too soft, was broken down with the couching needle, and its capsule freely ruptured to admit the aqueous humor. When the eye had recovered, a puncture was made in the cornea, through which the lens was removed in a soft state by the curette, and the speck and opake capsule were extracted, partly by the hook, partly by the forceps. The pupil became quite clear, and the girl left the hospital with as good sight as patients usually enjoy, after the removal of the crystalline lens."

Two plates accompany these observations; one represents the instruments employed by Mr. Gibson in the operations described, the other shows the appearance of the artificial pupil in the inner margin and centre of the iris. The volume, we are confident, will form a valuable addition to the library of the surgeon.

Observations on the Natural History, Climate, and Diseases of Madeira, during a period of eighteen years. By William Gourlay, M. D. Fellow of the Royal College of Physicians, Edinburgh; and Physician to the Factory at Madeira. 8vo. pp. 158. Callow, 1811.

From the Medical and Physical Journal for May 1811.

From the opportunities for observation and practice which the author's long residence in Madeira has procured him, we anticipated copious and valuable information respecting an island of which we hear much and know little. When chagrined with want of success, and wearied with the progressive increase of all the patient's symptoms, we propose a voyage to Madeira as a last hope, although we are no f urtheracquainted with the country, than having some confused notion that the climate is mild.

The first part of this volume treats of the discovery, situation, aspect, and soil of the island; of its vegetable and animal productions; and of the constitution, customs, and manners of the inhabitants.

"Madeira is situated in 32 degrees, 37 minutes, 30 seconds, north latitude, and in 17 deg. 5 min. longitude, west of Greenwich, about 80 leagues N. by E. from Teneriffe, 120 leagues from Cape Cantin, on the coast of Africa, nearly 100 leagues from the Isle of Terno, and about 17 leagues S. W. from Porto Santo. It is about 120 miles in circumference, its greatest length from E. to W. being 45 miles, its greatest breadth from S. to N. 15 miles, and its least breadth $8\frac{1}{2}$ miles. It is formed of lofty mountains, of hills, and fruitful valleys, and in figure makes an oblong irregular quadrangle. Its capital is Funchal, which is

situated on the south side of the island at the bottom of a spacious valley, open to the sea, and surrounded by lofty mountains, having all the appearance of an amphitheatre, gradually ascending to a great height. Its mountains and hills generally rise with a slow ascent; the highest points of land being about 8250 feet, or one mile and a half, above the level of the sea. The situation of Madeira, in some places, presents a most picturesque and enchanting appearance, while in others, huge perpendicular rocks, lofty precipices, prominent ridges, deep excavations and chasms, innumerable cascades liberally supplied with rivulets, beautiful valleys, deep gulleys and ravines, containing immense torrents of water, afford a highly varied, sublime, and no less alarming picture of nature." P. 5—6.

This island was formerly extremely fertile, and though the soil has been exhausted by a succession of crops for nearly four hundred years, without much assistance from culture it still nourishes a variety of vegetable productions; all sorts of tropical, as well as European fruits, will flourish on this favoured spot, and every kind of grape grows there.

Cattle, poultry, fish, &c. &c. are very plentiful; and various species of small birds are seen. We wish that Dr. Gourlay had enabled us to detail more particulars of the natural history of Madeira; his information on this head is very scanty, although many rich and curious productions of nature are to be met with in the island.

His account of the inhabitants is interesting.

"The natives of Madeira, particularly the peasantry (he observes) are distinguished by an olive or tawny colour of skin, and a swarthy complexion; nor is it improbable that they are of a mulatto or Moorish origin. Indeed only a few of the first families at all resemble in complexion the fair inhabitants of northern Europe, and these are unequivocally of Portuguese extraction. The people of Madeira are, in general, athletic, well made, active, and of a middle stature. Those of the lower class, or the country people, are sober, inoffensive, economical, and capable of enduring much hard labour; in the prosecution of which they are often reduced to great emaciation of body, and debility of constitution, and thus a premature old age is

brought on. The higher classes, on the contrary, are inclined to corpulence, and at the same time more disposed to indolence, attended with a moroseness of temper, and disposition to melancholy: though sober in respect to drinking, they are apt too often to indulge in eating to excess; from this circumstance, joined to the sedentary life they lead, they become subject to a variety of chronic disorders, and also early arrive at a premature old age; nor is it to be concealed that of late years the use of spirituous liquors has become common here among all ranks, which has opened a new field for the production of a long train of maladies." P. 27—28.

The women marry early, are very prolific, and soon lose the charms of youth. Their habits are adverse to health, leading very sedentary lives, seldom going out but to church, and performing many religious exercises. The younger branches of the first families are immured within convents, from which, after taking the veil, they never issue.

The climate of Madeira forms the subject of the second part of this work, and extracts from a Meteorological Register, kept during a period of sixteen years, are inserted. The temperature appears to be pretty uniform, the hottest months being rendered pleasant by a succession of land and sea breezes. The following table from Kirwan's "Temperature of different latitudes," presents the average temperature of every month in the year.

"Madeira, Funchal, lat. 32 degrees 37 minutes, long. 17 degrees, mean height of the thermometer for every month, taken from an average of four years' observations.

Jan.	64,18.	July,	73,45.
Feb.	64, 3.	August,	75,02.
March,	65, 8.	Sept.	75,76.
April,	65, 5.	October,	72, 5.
May,	66,53.	Nov.	69,08.
June,	69,74.	Dec.	65.

The following is the average temperature of Madeira, compared with that of London, for the whole year, as well as during the coldest and warmest months, which are January and

July. Taking the average temperature of London at 1000, the heat of Madeira is 1319. In January 0559, July 1128."

The third part treats of the diseases of Madeira; these the author arranges under the heads endemic and epidemic.

The first division comprehends various affections of the skin, complaints of the chest, rheumatism, colic, tetanus, dropsy, &c. Dr. Gourlay begins with describing elephantiasis, or Arabian leprosy, which is very common among the lower classes of people in Madeira. The disease is hereditary, and may be communicated from one person to another, although it does not appear to be of a very contagious nature.

" It generally shows itself by tubercles upon the face and upper extremities, and sometimes upon the trunk of the body and penis. Illconditioned ulcers of the legs also take place, in some instances, attended with acute pain; large indolent glandular tumors occupy the upper and anterior part of the thigh. The fingers become contracted, and the feet hard and swelled. The fingers also, and toes, are occasionally destroyed by ulceration; the same disposition to irregular tumors and ulceration attacks the throat.

" In those affected by the disease, previous to the age of puberty, the usual signs that mark this period of life do not appear. The beard, the usual sign of virility, is wanting; the hair is deficient on the pubis and scrotum, as well as on the axilla and breast. No desire prevails for the venereal passion: the voice preserves its puerile tone, and does not acquire the real strength and masculine expression. Even the testicles, not called upon for the exercise of their functions, gradually waste." P. 68, 9.

Young females affected with this disease experience no increase or fulness of the breasts, no growth of the external parts of generation, no appearance of the menses, have no hair on the pubis or axilla, and feel no disposition to venery.

" Even where the disease makes its first attack, at a much later period, the marks of sexual maturity, which are already established, gradually disappear, and are attended with impotence or very impaired powers of generation.

These circumstances denote that the elephantiasis of Madeira differs from the same complaint in other countries: for almost every writer on the subject has described the unhappy victims of this disease, raging with insatiable irresistible desire for venery; *rabiosa inest libido.* In support of the contagious nature of this disease, Dr. Gourlay states, that,

"About thirty years ago, in the village Ponta de Sol, fourteen miles distant from Funchal, the capital of the island, (Madeira) it raged with all the violence of an infectious malady, passing from one family to another, and threatening to extend its ravages into the neighbouring country, till the governor took this (the) prudent and wise step of separating the healthy from the diseased, and preventing their interference with each other." P. 72.

Some cases also under the author's immediate notice are cited in favour of this opinion. Where it affects people who have previously been healthy, the mode of attack resembles that which distinguishes contagious diseases. "It is ushered in with rigors and other symptoms of pyrexia, while at the same time there is no evidence of the presence of any other existing morbid cause." We have hitherto regarded this disorder as an incurable one; Dr. Gourlay however has occasionally succeeded in affording relief. In the incipient state, he has in a few instances suspended the progress of the disease by the use of calomel. In other cases he found great benefit follow the internal administration of the lacerta (agilis) or common lizard.

"As a medicine, this reptile acts as a powerful stimulant on the living solid, opening the several excretions, and producing large evacuations, particularly by the skin and urine, which are at the same time not attended with any debilitating effect. By this mode of operation, it will be found to have the certain influence of arresting the progress of the worst symptoms of elephantiasis, if not the whole, and in many cases to have surprisingly restored parts, which for years had been morbidly enlarged, to their natural size, and even sensibility; though for that period they had continued in a torpid state. Its operation also seems to vary somewhat in different cases; at times

the different secretions seem increased by it all at once, viz. the perspiration, urine, and saliva; at other times, merely an increase of saliva takes place." P. 74.

Diarrhœa sometimes followed the use of this remedy, and it occasionally produced vertigo. Some successful cases are subjoined from which we are convinced that this reptile possesses considerable efficacy in the cure of elephantiasis: It has also been administered with advantage, in herpes, chronic rheumatism, dropsy, and scrofula.

As most of the diseases in Madeira are similar to those of this country, we shall confine our extracts to such as present unusual appearances, or have some peculiar claim on our attention. Dr. Gourlay mentions an inveterate species of psora, termed by the natives, ouçao. It is well described by Dr. Adams in his work on morbid poisons. This affection usually attacks children at an early age, and is evidently occasioned by small animalcules piercing the skin. They possess the power of leaping like fleas; are somewhat larger than cheese mites, and belong to the genus acarus (siro exulcerans). Linn.

"The first appearance of the disease is in the form of a pellucid watery vesicle, attended with intolerable itching, and which, on being rubbed, breaks and discharges a thin watery fluid. A crust or scab is afterwards formed, from under which there is again emitted an acid ichorous matter. This matter corrodes the neighbouring parts, and tends to extend the disease, which is further assisted by the ova of the original infectious animalcules, and the locomotive power they are ascertained to possess."

The cure is most frequently performed by means of extraction with a pin or needle; but the complaint is easily removed by the application of mercurial and sulphur ointments.

"From a similar cause of animalcular irritation is derived another cutaneous malady, known by the name of alfora, from the small winged insect which occasions it. This insect is about the size of the one described, as producing the itch, and the affection it intails is most troublesome during harvest, and immediately after it." P. 85.

From cutaneous affections, our author proceeds to diseases of the chest, which in spite of its boasted salubrity are common in Madeira; the natives even are often affected with pulmonary consumption.

"Persons of all ages, and of both sexes, fall victims to it; nay, whole families have at times been suddenly swept away by it.

"The species of the disease that produces the ravages here, is that connected with scrofula, a disorder equally common here as in the colder regions of Europe: it uniformly at first assumes the appearance of a mild catarrh, but afterwards, when the real pulmonary symptoms commence, they prove more violent and rapid in their progress, than in the phthisis of northern climates."

An idea prevails in Madeira that this disease is of a contagious nature, and on that account the inhabitants of Funchal will hardly under any circumstances receive a phthisical patient into their houses.

The proper period for consumptive persons to leave England is the month of October, the fittest season in Madeira being from November to the beginning of June. In the treatment of phthisis, Dr. Gourlay experienced the most success from a cautious administration of digitalis. In the incipient stages, he generally effected a perfect cure; in the more advanced stages great relief, and in some instances even complete restoration to health, by the use of this remedy.

In the second division, epidemics, the author classes those diseases which arise from a specific contagion. Fever is frequent in the island, and it usually assumes the typhoid character. Dr. G. attributes the fatality of this complaint to bad practice, and especially to bleeding. This is often the case in the country; but in towns, under regular practitioners, the termination is generally favourable.

In 1806, scarlatina was epidemic the first time in Madeira, and attended with a great fatality.

"The characteristic symptoms which marked its attack, were inflammation of the tonsils, and mucous membrane of the fauces, attended with extensive and repeated sloughing of

these parts; eruption of the skin, varied in its appearance, form and extent, in different cases, and great debility of the whole of the functions. The affection of the throat, however, was by no means a constant symptom, and the attack was as frequently without it as with it.

"At its commencement, so contagious was the nature of this epidemic to appearance, as to be considered as the epidemic or contagious catarrh combined with quinsey, and in other cases, as measles; and indeed from the very variable mode of its attack, though its nature soon ceased to be in the least doubtful to an experienced practitioner, still it could not fail, from its incipient appearance, to deceive one who looked only to the regular and usual form of scarlatina. In many cases, for three or four days, delirium was the only symptom of the disease, attended with anxiety of the precordia, dyspnœa, palpitation of the heart, cough, bilious vomitings, edematous swellings of different parts of the body, and, in proportion to the violence of these symptoms, suspension also of sense and motion. In other instances, the malady was ushered in by violent hemorrhage from the nose and mouth, attended with a quick feeble pulse, and occasionally frequent fits of syncope." P. 111,112.

It chiefly affected children: we would willingly transcribe this author's very minute account of the symptoms, in the enumeration of which, he has evinced great accuracy of observation; but we have already rendered this article long. The treatment pursued seems to have been judicious, and we were gratified to find that in the cases where cold affusion was freely administered, considerable relief was afforded.

In the autumn, dysentery generally prevails, and is highly contagious. Smallpox was formerly very destructive in this island, but the practice of inoculation has now checked the mortality; and vaccination promises to be successful in further diminishing it. Dr. Gourlay states, that he has not failed in a single instance in producing the disease, and that none of his patients, though afterwards subjected to the contagion of smallpox, have been affected by it.

Measles are frequent among the children in Madeira; the inflammatory stage is very short, and soon succeeded by a de-

gree of typhoid debility, from which the chief danger arises. The submaxillary glands often swell and prove troublesome. Hooping-cough is epidemic, and the paroxysms are so violent as frequently to terminate in apoplexy. Emetics administered in the commencement of the complaint pro ce the most benefit.

An appendix contains a short account of the mineral waters in the Portuguese Island of St. Miguel.

Upon the whole we have been gratified by the perusal of this little volume; should a second edition be called for, we hope the author will endeavour to make the department allotted to the natural history of the island more complete. We cannot avoid also remarking, that besides a full page of errata enumerated, various other errors, both of the press and of the editor, have escaped correction.

ORIGINAL PAPERS.

Some account of the use of the Polygala Senega in obstinate Amenorrhœa.

BY N. CHAPMAN, M. D.

It has been remarked by the celebrated Dean Swift, with his usual propensity to epigrammatic point, that he who raises two sprigs of grass where only one had previously grown, confers on his country a more substantial benefit than is derived from all the achievements of arms.

To the same species of praise, and certainly not to a less share, is the physician intitled, who, by new discoveries, adds to the catalogue of means by which the maladies of our nature are mitigated or cured.

Every practitioner of medicine, who is much consulted in the diseases of women, must have experienced the difficulty of managing some of the forms of amenorrhœa, by the ordinary remedies. In a conversation with my friend Dr. Hartshorne, about two years ago, upon this subject, he mentioned to me that he thought he had used, with advantage, a decoction of the Senega in these cases. Confiding in the accuracy of his observations, I determined to lose no time in making a trial of the medicine. This I have since done, both in my private practice and in that of the Almshouse,* to a considerable extent, and with sufficient success, to warrant me in recommending it as among the most active and valuable of the emmenagogues. The ensuing cases, selected out of a much larger number, will serve not only to attest the decisive efficacy of the medicine, but may, perhaps, also, in some measure, illustrate the particular state and circumstances of this deranged function, to which it is best adapted.

The first case in which I gave the decoction, was that of a woman, aged thirty years, by trade a sempstress, of a

* The Almshouse, as the name imports, is an asylum for paupers. There is, however, a department of the establishment appropriated to the sick. This forms an extensive infirmary, and is among the best clinical schools in the United States.

frame slender and emaciated, and with a temperament apparently cold and leucophlegmatic. The disease had existed in a greater or less degree for ten years. At the commencement, and long afterwards, she menstruated irregularly, as well to the time, as the quantity of the discharge, and always with pain and difficulty. But for the three last years, there had been an entire suppression. Still, however, she occasionally felt, once perhaps in two or three months, "as if nature were making an effort." At these times, she had for days together, fugitive pains in the back, and lower part of the abdomen, accompanied with the uneasy sensations of fulness in the chest and head.

As almost every other remedy had been employed by the physicians who had previously attended the woman, I did not hesitate to put her on the use of the Senega. I directed her to take of the decoction* a table-spoonful every two hours, and to live on a diet generous and nourishing. No immediate effect from this plan of treatment was discernible. In about three weeks, however, she confessed that she was better, both as regarded the acquisition of strength, and the general comfortableness of her feelings. Towards the expiration of the fifth week, I was requested, in the evening, to visit her. I found her complaining of very acute pain in the stomach like the colic. Her bowels being opened by an injection, she took a large dose of laudanum. In the morning, I learnt that she had been speedily

* In making the decoction, I put a pint of boiling water to an ounce of the Senega, bruised, in a close vessel, and let it slowly simmer over the fire till the quantity is reduced one third. Where the medicine excites nausea, which it is sometimes apt to do, even in small doses, I have it prepared with the addition of an aromatic, such as the orange peel or cassia. My rule, in the administration of the Senega, is to give about four ounces of the decoction, more or less, during the day, according to the circumstances of the case. But at the time, when the menstrual effort is expected to be made, and till the discharge is actually induced, I push the dose as far as the stomach will allow. I have given as much as two ounces every hour. In the intervals of the menstrual periods, I always lay aside the medicine for a week or two. Without these intermissions, if it does not lose its efficacy, it becomes nauseous and disgusting to the patient. While under a course of the Senega, the general system is to be kept properly regulated. Excessive excitement, or debility, is to be equally obviated by the use of the appropriate remedies.

relieved, by the anodyne, of the colic, but that she was at the moment, suffering from strong bearing down pains. She seemed also to be distressed by great restlessness and anxiety. As her pulse too, was somewhat full, and her skin warm, eight ounces of blood were drawn from her arm. The medicine was now increased to an ounce every hour. When I saw her again in the evening, no very sensible change had taken place. I prescribed the warm bath, and the continuation of the medicine. The next morning she was evidently better. The bath, the over night, had composed her; and after a few hours' sleep she began to menstruate. The discharge, however, was by no means natural. It looked like a mixture of the sanies of an unhealthy ulcer, and the fluid of leucorrhœa. During the day and succeeding night, the medicine was continued. Part of this time, the discharge was more abundant, and of a different appearance. It consisted of pretty tenacious mucus, so blended with grumous blood as to have a dark sanguineous complexion. The menstrual paroxysm now ceased. After a fortnight, she returned to the Senega, in the dose of an ounce four times a day, and persevered in thus taking it for three weeks, when she was again attacked. I was not called to see her till the third day of her illness. It appeared that she had been affected precisely in the same way as formerly: in the first instance being seized with colic, which was soon followed by bearing down pains. These lasted, with slight intermissions, for twenty-four hours, when a substance of an unusual aspect coming from her, I was sent for to examine it.* I found it to be a membranous lining of the uterus. It was exactly of the triangular figure of the inside of that organ, and of the dimensions of its cavity at the close of the second month of gestation. The external surface of it was shaggy or flocculose: the internal smooth. In consistence, it resembled firmly impacted mucus, or that structure denominated by anatomists *parenchyma*. In color, it was similar to the decidua, but twice or three times

* I ascertained that the Senega, in an increased dose, was the only medicine which had been given prior to my visit. No other remedy was afterwards employed during this attack.

as thick as I have ever seen that membrane.* The expulsion of this substance was neither preceded, nor followed, by hemorrhage, or other discharge, except a weeping of serum tinged with red blood.†

The woman, after a short interval, once more reverted to her medicine: and in the course of three weeks, I received the very satisfactory intelligence that she was *menstruating naturally*. Three cloths, which were shown to me on my visit to her, were thoroughly imbued with the genuine catamenia. Eighteen months have elapsed since this event, and she continues to do well, without any medical aid. The menses flow freely, but with no sort of periodical regularity. In every respect, however, her health has vastly improved.

I have traced the more minutely the history of the above case, because it strikes me as being interesting in several points of view. It at least corroborates an opinion which I had reason early to adopt, that in most, if not all the instances of obstinate and long continued amenorrhœa, the membrana decidua exists;‡ and that radical relief, under such circumstances, is to be alone sought in the riddance of this adventitious production.

The cases that follow, came under my care while in attendance at the Almshouse last winter. They will be more briefly related. Disappointed in procuring the records which are preserved, or ought to be preserved, in the clinical journal of the

* Morgagni was the first, as far as I know, who noticed this membranous substance. He describes it very accurately. By Denman our attention has been more recently called to the species of amenorrhœa to which it is incident. He mentions, among many other particulars relating to this substance, that he has known it to be generated, in more than one instance, by the virgin uterus. Such a case has never occurred to me.

† There is commonly in these cases, as much hemorrhage as in early abortions. It is said by Morgagni, that the expulsion of the membrane is succeeded by the lochial discharge. I have never observed it.

‡ Dr. Denman seems to entertain the same opinion. He, indeed, suspects that this membrane exists in every case of "habitually painful menstruation." There are two reasons why it is not more frequently observed. Women, in the first place, are reluctant to have their menstrual cloths inspected; and secondly, the membrane, in a great majority of instances, comes away in small pieces, or flakes, so that it may be overlooked, even when we have opportunities of an examination.

house, I am compelled to rely exclusively on my memory, which, at this interval, enables me to draw up only a general statement.*

CASE I.

The woman was a maniac. She had been confined to her cell for upwards of a year. During the greater part of this long period, she was furiously deranged. Of the cause of her insanity nothing could be ascertained. Her age appeared to be about twenty-five years.

I found that she had been most copiously depleted by the several physicians who preceded me. Bleeding, both general and topical, purging, blistering, and salivation, had all been pushed to a great extent. Besides these remedies, antimonial preparations, the digitalis, the decoction of tobacco, and a variety of *emmenagogues* had been tried. But, nevertheless, no very favorable impression had been made on her disease. Being told that she had not menstruated since she had entered the house, I determined, without delay, to endeavour to restore this function. I accordingly directed that she should take the decoction of Senega, in the accustomed dose; occasionally to lose some blood, and to use the tepid bath. After a few days her situation evidently meliorated; and at the end of some weeks, a flow of the catamenia came on, which was productive of the most salutary consequences. No morbid action now disturbed her system, and her mind shared in this new state of tranquillity. Encouraged by a change so propitious, the medicine was continued four or five weeks longer. Then menstruating a second time, she became still better, and her friends, considering her sufficiently recovered, took her home. Of late I have not heard of her: but for some time after leaving the house she had no relapse.†

* My friends Drs. Wallace and Morrice, of Virginia, who I believe took notes of these cases, will, I hope, correct, if they detect, any material inaccuracy in my statements.

† It may be proper for me to state, that though, when she quitted the house, she was cured of mania, yet her intellect, by the long derangement which it had suffered, was much impaired. It was, indeed, debilitated almost to fatuity. I understood, however, that her mind afterwards recovered somewhat of its tone.

CASE II.

This was also the case of a maniac. In many leading features it resembled the preceding one. The woman was about the same age, and her derangement of equal standing. In both instances the catamenia were suppressed. The treatment had been similar. There was this difference, however, in the state of the two women, on my first seeing them. The mind of the latter was far more sane. There was too, a strong tincture of lewdness in her aspect, her conversation, and deportment. This distempered ardor of appetite most rapidly increased. In less than a week she had confirmed nympho-mania, associated with extreme mental vivacity, and apparent joyousness of heart. Taking the whole of the circumstances of this case into consideration, I had little doubt as to the practice which ought to be pursued. My determination was promptly made up, to arouse the uterus, if possible, to a catamenial secretion. I was persuaded that a flow of the menses would most effectually quell this turbulence of passion, and probably likewise, as in the other case, alleviate her mental disorder. With the decoction of senega I therefore commenced. Of this she took, perhaps for a fortnight, without any visible advantage. The dose was in consequence augmented to two ounces every hour. At my next visit, three days afterwards, I was struck, on entering her cell, by a very extraordinary alteration in her appearance and demeanor. Instead of the eager and impassioned salutation which I had been in the habit of receiving from her, she remained on her bed tranquil and sedate. There was no longer any inordinate excitement. Composed both in body and mind, she seemed to be indulging a species of melancholy contemplation, from which it was not easy to divert her.

Inquiring of the nurse what had so suddenly revolutionized the condition of my patient, I was informed that she had been *menstruating copiously for the last thirty-six hours.*

As her health continued progressively to mend, I intrusted the case entirely to the powers of nature for two or three

weeks.* But I was constrained again to resort to the senega, in consequence of some menacing indications of a relapse. The medicine very quickly produced the anticipated effect. After menstruating freely, she became convalescent as before. My period of attendance in the house now expired, and she was consigned to my successor. What was the ultimate result of the case, I am unable to say.

To avoid prolixity, I shall dismiss these cases with one or two remarks only, though they are rich in practical reflections. Their importance is not restricted to the evidence which they bear to the powers of the senega as an Emmenagogue. They may, perhaps, open a new view in the treatment of mania in females. If they do not conclusively evince that amenorrhœa is sometimes a cause of insanity, they at least are calculated to awaken our suspicions upon the subject. Nor is this all. They show distinctly the intimate relation between the two diseases, and demonstrate, in the most striking manner, how essential it is in treating the affections of the mind, to have a constant and vigilant eye directed to the uterine function. They prove, in short, incontestably, that in some cases of mania in women, a cure may be effected, when other means have totally failed, by simply producing an effusion of the menses.

It now only remains for me, very concisely, to suggest the mode in which I conceive the senega to operate. Inquiries of this nature are not without utility. They lead to the establishment of principles, by which we regulate our practice with greater precision and certainty.

The menstrual discharge I hold to be *the result of a secretory action of the uterus*. Every other hypothesis on the subject is totally irreconcilable with facts, and repugnant to the laws of the animal economy. The crude speculations of former times respecting this operation may, indeed, be considered as discarded. Does any one, for instance, whose knowledge has kept pace with the improvements of physiology, now entertain the notion of lunar influence, of fermentation, of venereal ap-

* Since writing the above, I recollect that blisters, at different times, were applied both to the inside of her thighs, and ankles, and kept open during the greater part of this period.

petite, of general plethora, or local congestion, &c. &c.? The theory of secretion I am aware is not fully established. But much may be urged in its support. My limits, however, will permit me at present, to bring to its defence merely a summary of the more leading arguments. These may be thus enumerated.*

1. That the uterus in its villous, and vascular structure, resembles a gland, and also in its diseases, being equally liable to scirrhus, cancer, &c. &c.

2. That, like other secretory organs, blood is very copiously diffused through it.

3. That, by the arrangement of its vessels, it is evidently designed that the circulation should be retarded for the *purpose of secretion.* The arteries are not only exceedingly convoluted, but they are larger and with thinner coats than their corresponding veins.†

4. That in common with other secretions, menstruation is often, at first, imperfectly performed, and is subject afterwards to vitiation and derangement. In the beginning the discharge is commonly thin, colorless and deficient, and recurs at protracted and irregular intervals. In some of these particulars, it is analogous to the seminal secretion.

* Who originally suggested the theory of secretion, I have not been able to ascertain. It has very generally been ascribed to the celebrated Mr. Hunter; but the evidence of his claims to it is exceedingly slender. The only trace of it, which I can discover in his writings, is a vague expression in a paragraph of his Treatise on the Blood. Afterwards, however, he furnished an extract from his lectures to be published in Johnson's Midwifery, as exhibiting more fully his notions respecting this function. Speaking of the death of the blood from lightning, and other sudden causes, he includes the catamenia among the illustrations of his reasonings. "The blood," says he, "discharged in menstruation, is neither similar to blood taken from a vein "of the same person, nor to that extravasated by an accident in any other "part of the body: but is a species of blood changed, separated, or thrown off "from the common mass by an action of the vessels of the uterus, in a pro-"cess *similar to secretion*, by which action the blood having lost its living prin-"ciple does not coagulate," &c. I am told that the theory occurs in an old French system of physiology. It was certainly published in a thesis, at Edinburgh, by Dr. Craven, in the year 1778.

† "The blood," says Haller, "is brought to the womb in greater quanti-"ty, and more quickly through its lax and ample arteries, and on account "of the rigidity and narrowness of the veins, it *returns with difficulty.*"

5. That in many of the inferior animals, during the season of venereal incalescence, there is an uterine effusion which is undoubtedly *a secretion*. This answers seemingly the same end as menstruation, namely, giving to the uterus an *aptitude to conception*. Though this fluid generally differs from the menses in complexion, yet in some instances, they are precisely similar. Whenever the venereal desire suffers a violent exacerbation from restraint, or other causes, the discharge in these animals becomes red. This has been more especially remarked in bitches kept from the male.

6. That when the menses are suppressed, they cannot be restored by inducing plethora, nor the flow be checked by blood-letting, or any other means of depletion. Besides, no vicarious discharge relieves the symptoms of suppression! Do not these facts very unequivocally proclaim the existence of a secretory function?

Lastly. That the menses are a fluid *sui generis*, or at least varying very essentially from blood; having neither its *color*, nor *odor*, nor *coagulability*, and on chemical analysis present *different results*. Let me ask, if the menstrual fluid be *not blood*, what is it?

To the objection, which has sometimes been urged, that the uterus is not sufficiently glandular for the office alleged, it may be, I think, very satisfactorily replied, that there is hardly a viscus, or surface of the body, which is not competent to this purpose. It would really seem that no operation of the animal economy requires a less complex apparatus. Of what, indeed, does a gland consist, except a congeries of vessels? Even the most perfect of the secretions are accomplished by this simple contrivance. If a few vessels, "creeping through the coats of the stomach," can secrete the gastric liquor, why may not the infinitely more glandular organization of the uterus elaborate the menstrual fluid?*

* As yet we know of no glandular structure in vegetables. They contain only tubes or vessels, through which the fluids circulate. Notwithstanding, however, the want of glands, we find the sap of plants converted into oil, mucilage, acids, &c. No stronger proof can certainly be required of the extreme simplicity of the organs by which the secretory transformations are effected.

By admitting the truth of my position, we have, at once, a solution of the problem before us. It follows necessarily from the concession, that amenorrhœa is caused by an interruption to the secretory action of the uterus; and that the remedies which remove the suppression, operate by bringing the organ back to that precise condition, on which the exercise of this function depends.

No one, who has carefully watched the effects of the medicine upon the system, but must have perceived how actively it promotes the various secretory discharges. This is especially manifested in the glands of the mouth and fauces, and the neighbouring secretory surfaces. Excepting the mercurial preparations, I doubt exceedingly whether we have any medicine so eminently possessed of the property to which I allude. Unquestionably there is none that so uniformly excites ptyalism.

Deny to the senega the specific energies for which I contend, and where shall we seek an explanation of its effects? If it be simply a stimulant, or tonic, or sudorific, as is more generally supposed, it might kindle excitement, or impart tone, or raise a diaphoresis, like many other articles of the materia medica; but would it be so signally efficacious as an Emmenagogue?

The doctrine of *specific actions* is not new. It once prevailed in the schools of physic, but has been supplanted by hypotheses which are, in my opinion, far more remote from the truth. The bold assumption that all medicines are similar in their operation, differing only in degree of force, permanency, and diffusibility, however plausible in speculation, will not bear the test of facts. The directly opposite conclusion is abundantly confirmed by experience.

I believe, that scarcely any two agents produce precisely the same effects upon the living system. Hence the *variety of diseases*, and the necessity of a *multitude of remedies*. Not the slightest of the causes, which conspire to retard the progress of physic, is the eagerness to generalization, that at all times has distinguished its cultivators. Of late, however, this propensity has been carried to the utmost extremity. Disease is now held by an eminent teacher to be *a unit;* and all medicines are considered, by the disciples of another, as "participant of one effect." Such hypotheses may have the semblance of the simpli-

city which they claim, but, in *reality*, they are "confusion worse confounded."

If we are ever to strip our art of its "glorious uncertainties," and to introduce into the practice of it something of exactness, it will be by pursuing a very different course. To accomplish so important a revolution, we must diligently, and without prepossessions, observe the *phenomena of disease*, and, with an attention no less unbiassed, investigate the *properties of medicines*. Thus, perhaps, we shall ultimately learn to distinguish accurately the *diversified shades* of morbid action, and to apply to each its *appropriate* remedy. As it is, however, at present, we are plunged into a Dædalian labyrinth without a clew. Dark and perplexed, our devious career, to borrow the fine illustration of a favourite writer, resembles the blind gropings of Homer's Cyclops round his cave

But to return from this digression. In all the forms of amenorrhœa, I suspect the senega may be useful, if administered with a proper regard to the state of the system, and in other respects, with correct discrimination. But it promises to be more particularly so in those cases where the decidua exists. We are, as yet, ignorant of the exact process by which the membrane is framed. Of this, however, there is no doubt, that the vessels of the uterus, which pour out the catamenia, are the instruments by which it is done. Nor is it less certain, that while they are engaged either in the formation or support of this new production, menstruation ceases. The two offices, indeed, exact modes of action totally incompatible. It is obvious, that under these circumstances, to excite the uterus to a secretory effort, not only a *forcible*, but a *specific impression* must be made upon it. I am entirely persuaded, both from my speculative views of its properties, and my actual experience, that the powers of the senega will be here most conspicuously displayed. Should my sanguine expectations of its efficacy in these cases be fully confirmed, it will indeed be an important acquisition. This species of amenorrhœa is, on many accounts, so serious an evil, and has hitherto proved so difficult of cure, that a remedy for it is, confessedly, among those things the most anxiously desiderated in the practice of physic.

Philadelphia, September 20, 1811.

P. S. I received the annexed communication after my own paper was committed to the press. I am glad to have it in my power to furnish such additional evidence, and from so respectable authority, to the efficacy of the senega.

DEAR DOCTOR,

I send you an account of my experience in the use of the senega polygala in suppression of the menses, to Dec. 1809. Since that period I have frequently prescribed it, but have not kept accurate notes of its effects.

My confidence in the efficacy of this medicine is by no means diminished.

Respectfully, &c.

J. HARTSHORNE.

Dr. N. Chapman.

I have used a saturated decoction of the senega in twenty-three cases of amenorrhœa. In thirteen of these, the menses appeared during the use of the decoction; in two, the health of the patients was very much improved, but the catamenia did not return; in three, no advantage whatever was derived from it. In the remaining five cases, I have not been able to ascertain the event. In the last case in which I used this remedy, the chlorotic symptoms were very violent. The menses had been suppressed eight months. From twelve to sixteen ounces of the decoction were taken in the course of twenty-four hours, for two weeks; the menses then appeared, and the patient has since enjoyed much better health.

The senega has sometimes induced a flow of the menses, without alleviating the disease which accompanied the suppression.

I have always wished to commence the exhibition of this remedy at least two weeks before the period when the patient had usually menstruated. It produces no mischievous effects, and might perhaps be exhibited with safety for a much greater length of time. My experience however does not warrant an assertion that its long continued use would not be injurious, as none of my patients have continued it more than two weeks.

An Analysis of the Water of the Sulphur Springs, situated in Adams County, Pennsylvania.

BY JAMES CUTBUSH,
Lecturer on Chemistry, Philadelphia.

THE following experiments were made with the water brought from the spring by Mr. John Greiner, which was bottled, corked, and bladdered.

1. *Examination by Reagents.*

EXP. 1. Alcohol of galls did not produce a change, nor did prussiate of potash, nor succinate of ammonia.

2. Muriate, as well as acetite of barytes, produced a precipitate insoluble in muriatic acid.

3. Lime water produced no change.

4. Nitrate of silver gave a white precipitate.

5. Solution of soap occasioned a turbidness.

6. Oxalate of potash produced a precipitate.

7. Caustic potash gave a white precipitate soluble in muriatic acid.

8. Carbonate of potash produced a precipitate soluble with effervescence in muriatic acid.

9. Carbonate of ammonia was added to a portion of the water; the liquor was then filtered; and phosphate of soda, added to the clear liquor, gave a copious precipitate.

10. Sulphated silver gave a white precipitate.

The hepatic smell, of which the water partakes at the spring, was entirely lost by the conveyance; but, on examining the earthy matter taken from the bottom of the spring, which Mr. G. also brought, evident signs of sulphur in an uncombined state were discovered. The experiments already described prove the presence of several substances, namely:

Sulphuric acid, muriatic acid, lime, magnesia.

Exp. 1. proved the nonexistence of iron.

2. The presence of sulphuric acid.

3. The nonexistence of carbonic acid.

4. The presence of muriatic acid.

5. The existence of earthy salt.
6. The presence of lime.
7. The existence of earthy matter.
8. Do. do.
9. The presence of magnesia.
10. This experiment confirmed experiment 4.

Having thus pointed out, in a general manner, the fixed ingredients of the York spring, the next object of research was to determine the proportion of the magnesian and calcareous salts, and to ascertain if the muriate, which was detected, was of an earthy or an alkaline nature. The following section of experiments was therefore instituted.

II. *Examination by Evaporation, &c.*

Exp. 1. Two ounces, by measure, of the water was evaporated to dryness: the residue was carefully collected and weighed. It amounted to four grains. One pint, or sixteen ounces, therefore contains thirty-two grains.

2. To a portion of this powder, a few drops of sulphuric acid was added: muriatic acid gas was disengaged.

3. In order to ascertain the presence of muriate of soda, a portion of the water was evaporated until a film formed on the surface. The vessel was then removed to a cool situation. Small cubic crystals now appeared.

These experiments prove, that in sixteen ounces of the water, thirty-two grains of saline matter exists; and that it is composed of sulphate of magnesia, sulphate of lime, and muriate of soda. To ascertain the relative quantities of each, a number of experiments were instituted. The formula adopted for this purpose was agreeable to Klaproth. After separating the muriate of soda, the remaining portion, consisting of the two earthy salts, was treated with twice its weight of cold distilled water. The insoluble part was collected on the filter, and washed with a small quantity of water; it was then dried in a low red heat, and weighed. After thus separating the sulphate of lime, the soluble part, consisting of the magnesian salt, was treated with carbonate of potash; the precipitate (carbonate of magnesia) was washed, dried, and calcined.

Accordingly I found, that thirty-two grains of the saline matter contained

Sulphate of magnesia, (Epsom salt)	20
Sulphate of lime, (gypsum)	6
Muriate of soda, (common salt)	4
Loss,	2
	32

Or, very nearly,

Magnesia,	11
Lime,	3
Sulphuric acid,	12 grains.

Consequently, 11+3+12+4=30 grains; or the proportion of the ingredients, according to experiment, in one pint of the water.

The water at the spring, I am informed, is strongly hepatic; but in consequence of the carriage, it was entirely lost. Mr. Greiner, at the same time, favoured me with the earthy matter, said to contain sulphur, obtained from the bottom of the spring. On examining it, I found it strongly impregnated; for, on exposing it to ignited coals, it emitted a strong smell of sulphur. I judge from this phenomenon, and from the nature of the water, that sulphur exists in the neighbourhood, in union with earthy substances; and on their decomposition, no doubt, depends the formation of the sulphates already spoken of.

When this matter is taken from the spring, it is of a black color; but when it becomes dry, the color changes to a darkish gray. It is said to be useful in herpetic eruptions.

Hints on the Treatment of Insane Persons.

For the Eclectic Repertory.

THE following general hints on the treatment of insane persons have been chiefly drawn from the experience of *The Retreat*, near York (Eng.) The compiler however is fully aware, that after all which can be learnt from others, much must remain for experience to teach. The modes of insanity, and consequently its treatment, will be found to vary, in no small degree, with all the varying characters, manners, and habits of the human mind; and therefore the object, to which those concerned in the establishment of lunatic institutions ought most peculiarly to direct their attention, is the selection of a manager who possesses, in addition to other qualifications, a natural quickness of perception, and tendency to observe the varieties of mind, which will enable him to perceive with readiness, and apply with address, such moral treatment as the different cases may require.

Medical Treatment. It must be confessed, that the experience of the Retreat has not thrown much light on the medical treatment of insanity. It has however led the manager to believe, that but little is to be done by its aid, and convinced him of the folly as well as cruelty of forcing upon the patient a large quantity of nauseous draughts, at a time when probably his aversions are more than usually strong, and when he is with difficulty induced to take the food necessary for his support. The use of medicine is however far from abandoned at the Retreat. In all cases where the disease has supervened, or is attended by any obvious bodily disease, however slight, advantage may be reasonably expected from the removal of such complaint.

The warm bath has been much used at the Retreat for several years in all cases of *melancholy*, with the happiest effects. Indeed there has not been any recent case in which it has been employed without relieving or removing the complaint. The patient usually makes use of the bath every other day, and continues in it about twenty minutes, at a temperature of about

eighty-five degrees. The time of continuance is gradually advanced to nearly an hour, and the temperature to about ninety-six degrees.

The difficulty of obtaining sleep for maniacal patients, and that opium, though taken in large doses, frequently produces no effect, is well known. It suggested itself, however, to the sensible mind of the superintendent, that all animals in a natural state, repose after a full meal, and, reasoning by analogy, he was led to imagine, that a liberal supper would perhaps prove the most effectual anodyne. He therefore caused a patient, whose violent excitement of mind indisposed him to sleep, to be supplied freely with meat, or cheese and bread, and good porter. The effect answered his expectation, and this mode of obtaining sleep has since been generally successfully employed. In cases where the patient is averse to take food, porter alone has been employed with great advantage. Medical attention to the general bodily health, during convalescence, and in the lucid intervals of the patient, is of great importance. The return of the disease may frequently be suspended, if not prevented, by this kind of attention. The operation of cupping, applied to the head, temples, or neighbouring parts, has very frequently been attended with great advantage, where any pain or heat has been felt in the head. General bleeding and other evacuations have been found injurious at the Retreat, and are therefore not used, except where their necessity is indicated by the state of the bodily habit.

Air and Regimen. The situation of the Retreat is high, and affords a very dry air, which there is reason to believe is of very great importance to lunatics, but more especially for those of the melancholy class.

All the patients should, as much as their cases will allow, be exposed to the open air, and particular care should be taken that their different apartments be well ventilated.

A cheerful, pleasant scenery, in a retired situation, seems a desirable appendage to a dry and healty site for the building. A *single* range of rooms, with a gallery opening to the external view, is much preferable to a range of rooms on each side of a gallery. For though the exclusion of much light, in some violent cases, is found desirable, this is not a general circumstance.

Every *appearance* of a place of confinement should be studiously avoided. The window frames at the Retreat are cast iron sashes of a full size, with panes too small to admit of a patient's escape through them. Separate wards for the men and women patients, and the different descriptions of each sex, are also very essential.

The regimen of lunatics should be regulated by a consideration of the nature of the disease, and the state of the bodily habit. The experience of the Retreat has fully convinced the manager of the general impropriety of reducing the patient, however violent, by a low diet, or violent evacuations.

The following is the general diet of the patients, viz.

Breakfast. Milk and bread, or milk porridge.

Dinner. Pudding and flesh meat six days in the week; fruit, pudding, and broth, one day.

In the afternoon. The men have bread and beer, the women tea or coffee.

Supper is generally the same as breakfast; sometimes bread, cheese, and beer.

The parlour patients, when sufficiently well, partake of whatever comes to the superintendent's table, and some are supplied from it in their own apartments.

Moral Treatment. It will naturally occur to most persons, that the first objects to which the attendants upon lunatics ought to apply, is to obtain their entire confidence and good opinion, and for this purpose, it is necessary to treat them with uniform kindness, and never to deceive them, either by promises or threats. The idea, which has too much prevailed, that it is necessary to commence an acquaintance with lunatics by an exhibition of strength, or an appearance of austerity, has been found, at the Retreat, extremely erroneous, and to be a part of that cruel system, dictated perhaps by timidity, which has so long prevailed, and unhappily still prevails in many of the receptacles for the insane. Perhaps, in general, much familiarity, when a patient first enters the house, would have a tendency to lessen that authority which it is in some cases necessary for the attendant to possess and exert; but, let it be remembered, that in most instances of mental depression, the behaviour, though it ought to be firm, cannot be too kind, con-

ciliating, and tender. There may, however, be particular cases in which the attendant may, perhaps with advantage, assume a distant and somewhat important manner, but it must be done with extreme judgment, as the observation of maniacs is frequently morbidly acute.

It rarely happens that all the faculties are deranged at the same time, and the moderate exertion of those which remain sane, is calculated to correct and strengthen those which are diseased. The patient on all occasions should be spoken to and treated as much in the manner of a rational being as the state of his mind will possibly allow. By this means, the spark of reason will be cherished, and that painful feeling of degradation, which must be felt in a greater or less degree by all who recover the loss of their rational powers, and which cannot fail materially to depress them, will be greatly lessened. During the state of convalescence, attention to this hint is peculiarly important, and the greatest advantage has been found to arise at the Retreat, from introducing the patients who appear to be recovering, into the society of the superintendent and the other rational parts of the family. This liberty is afforded to the poor as well as the more opulent patients, as it has been found very materially to accelerate the recovery, if indeed it be not essential to it.

Of the modes of employing the minds of maniacs, those are to be preferred, which are accompanied by bodily action, which are of the amusing kind, and are most opposite to the illusions of their disease. When the patient is himself inclined to any particular employment, if not very unsuitable, it should be seized as a favourable circumstance, and carefully encouraged.

Conversation with those who can condescend to their weaknesses, and walks into the country, under suitable care have been found of essential benefit to the different classes of patients. The salutary effects of air and exercise, the variety and beauty of the objects of nature and rural life, as well as the subjects of conversation which these excite, have a strong tendency to still the effervescence of an overheated imagination, and to inspire the anxious and melancholy mind with tranquil and pleasing emotions. The experience of the Retreat has fully shown that, in melancholic and hypochondriac cases, close

confinement is of all things the most unsuitable. Hence we may explain, why so few cases of melancholy are cured in lunatic asylums in general, and hence, happily, we may also, in great measure explain, why so many labouring under these most affecting complaints have been restored, by their treatment at the Retreat, to their families and society.

Several of the convalescents are made useful in assisting the attendants, and the females in general are employed in needlework, where they can be safely intrusted with the necessary implements. The use of books and pen and ink are generally allowed, and the indulgence has been found generally beneficial.

It must however be allowed, that a suitable mode of employing the men patients is still a desideratum.

The principle of honour is often very strong in the minds of lunatics. I have known patients, who were under a voluntary engagement of good behaviour, hold a successful contest, for a considerable length of time, with the strong wayward propensities of their disorder, and even conceal all marks of aberration of mind. The attempt is highly beneficial to the patient, and ought to be sedulously encouraged by the attendant.

Several of the patients are permitted to attend the meetings in the city, and all who are suitable are assembled together on a first day afternoon, when the superintendent reads to them some chapters of the Bible. A profound silence ensues, during which, as well as the reading, it is curious to observe the order with which the patients conduct themselves, and control their different propensities. In cases where the patient is disposed to be violent, advice given in a friendly manner is very often successful in preventing the necessity of harsh measures, which ought never to be employed without absolute necessity, and then with obvious marks of regret on the part of the attendant. It is proper however to observe, that no advantage has been found to result from reasoning with maniacs on their particular hallucination. One of the distinguishing marks of insanity is a false conception, which of course occasions an incapacity of conviction. The attempt therefore generally irritates the patient, and rivets the false perception more strongly on his mind.

When a patient proves refractory, and coercive measures are indispensable, it is advisable to have an ample force em-

ployed, as it prevents in general any attempt at resistance; but where such force cannot be obtained, and the case is urgent, courage and confidence will generally overcome the violence of the patient, for there are hardly any instances in which maniacs have displayed true courage. The superintendent of the Retreat was one day walking, in a field adjacent to the house, with a patient who, when opposed, was apt to be vindictive. An exciting circumstance occurred; the maniac retired a few paces, and took up a large stone, which he held up as in the act of throwing at his companion. The superintendent, in no degree ruffled, fixed his eye upon him, and in a resolute tone of voice, at the same time marching towards him, commanded him to lay down the stone. As he approached, the hand of the lunatic gradually sunk from its threatening position, and dropped the stone to the ground. He then submitted to be quietly led to his apartment. One motion of timidity on the part of the superintendent might have cost him his life.

Modes of coercion. Kind yet firm treatment has been found to supersede the necessity of much coercion of any kind. Neither chains nor corporal punishments have *ever* been allowed on any pretext at the Retreat. The strait jacket, or a belt round the waist, which has straps that confine the arms close to the side, are the only instruments of coercion made use of when the patient is not in bed; when it is found necessary to confine the patient in a recumbent posture, the superintendent has invented a mode of fastening, which allows a change of posture, and the bending of all the joints of the body.

If in any instances chains are necessary for the confinement of lunatics, it is much to be feared that the highest pitch of maniacal fury has been excited by the cruelty or improper treatment of the attendants, very few of whom are fit to be intrusted with much power. It should therefore be as limited as it can be with safety, that they may be obliged to use every means, to govern rather by esteem than severity, and avoid every thing likely to exasperate the patients.

Inguinal Aneurism cured by tying the External Iliac Artery in the Pelvis.

BY JOHN SYNG DORSEY, M. D.

For the Eclectic Repertory.

On the 15th of August 1811, I was consulted by Alexander Patton, on account of a tumor in his right groin. The patient was a native of Aberdeenshire in Scotland, aged about thirty years, the last ten of which he had passed in America, having followed the trade of a cooper; was accustomed to hard labour, and to athletic exercises, jumping, running and the like. He was six feet in height, of a robust but not a corpulent habit.

Two years ago he perceived, for the first time, a small tumor in the right groin. Having never had the venereal, nor indeed any other disease, and not having met with any accident, he was at a loss to account for this appearance. From its commencement it throbbed with considerable violence. For a year and four months it increased very slowly; during the last eight months much more rapidly. In January it was no bigger than a walnut; in August its shortest diameter was four inches, its longest, nearly five. It occasionally gave him severe pain, and at length incapacitated him from all labour. In June last he applied to Dr. Irwin of Easton, the place of his residence, who instantly apprised him of the nature and importance of the complaint, and advised him to come to Philadelphia. He arrived here the 14th of August, and was admitted next day into the Pennsylvania hospital.

On examination, an aneurism was found, situated immediately below Poupart's ligament, forming a regular tumor in the groin, nearly hemispherical, with a kind of apex, where the skin appeared extremely thin, and discolored as if by ecchymosis. The patient had used a good deal of exercise previously to his admission into the hospital, and had taken a drink of rum, in consequence of which his arterial system was greatly excited, and the tumor pulsated so violently that the bed clothes were bounced up with great force. He was confined to bed; was purged, and kept to a low diet. A consultation was called;

and the surgeons of the house concurred in recommending the operation of tying the artery as high as practicable above the tumor. It was determined to perform the operation promptly; as the disease was progressing, and no benefit was to be expected from delay.

On Monday, 19th of August, at noon, in presence of Dr. Physick and Dr. Hartshorn, surgeons to the hospital, and a number of medical gentlemen, I proceeded to the operation. The patient, having previously taken fifty drops of laudanum, was placed on the table. An incision, three inches and a half long, was made, beginning an inch and a half higher than the superior anterior spinous process of the ilium, and one inch distant from that process internally; being also four inches and a half distant from the umbilicus, extending obliquely downward, and terminating about one inch above the basis of the tumor. This incision, which was nearly in the direction of the fibres of the tendon of the external oblique muscle, divided the skin and adipose membrane, and exposed that tendon, which was next cut through, the whole length of the external incision. The internal oblique muscle now protruded at the wound and was carefully cut through; the inferior edge of the transversalis abdominis was next divided, but not so far upward as the top of the external wound. My finger was then introduced, and the cellular texture readily yielded it a passage to the external iliac artery, the trunk of which I distinctly felt pulsating very strongly. With my finger I separated it gently from the neighbouring parts; but took care to denude only a very small portion of the vessel. The peritoneum I was equally careful to detach as little as possible; and not more than a square inch of it was disturbed. The only remaining difficulty in the operation was to pass the ligature round the vessel; and this having been anticipated, was readily surmounted. Before commencing the operation, I had secured an aneurismal needle (a blunt bodkin of silver properly bent) in a pair of curved forceps, by tying the handles of the forceps firmly together. The needle was armed with strong bobbin; and thus connected with the forceps, resembled a tenaculum, which could easily be managed outside of the wound. With one finger in the wound I found it very

easy to direct the extremity of the needle, and with the forceps in my other hand, to push it through the fascia surrounding the vessel. The string connecting the handles of the forceps was now cut, and the needle was left under the vessel. The forceps being removed, the needle was drawn out, leaving the ligature round the artery. Convinced, by careful examination, that nothing but the artery was included in the ligature, and that it was, to the best of my judgment, natural in size and texture, I tied it very firmly, as high up as possible. The pulsation of the tumor instantly ceased. Three knots were made, and the ends of the ligature were left out at the external wound. No bloodvessel of magnitude was divided, and not half an ounce of blood was lost. No stitches were employed to close the wound; a strip of adhesive plaster effectually answered this purpose. A pledget of lint was applied, and the patient was put to bed, his thigh being moderately flexed upon the pelvis. He complained of extreme pain during the latter part of the operation, the whole of which occupied eleven minutes.

The patient's pulse, for several days before the operation, was 80: after the operation it was 88, and rose in the afternoon to 100. At four o'clock he was bled ten ounces. At seven he complained of extreme pain in the back and belly, and also of some pain in the limb. He was not permitted to take any sustenance except toast and water. The superficial veins of the leg and foot were filled; and the whole of the limb was covered all the evening with perspiration. Its temperature was examined repeatedly by a thermometer, and was five degrees colder than the other. It was covered with flannel and carded wool.

Tuesday 20th. Passed a restless night, in great pain. To use his own language, in expressing his sensations, "he felt as "if his loins were tearing apart." He was also troubled with pain of the bowels. Three grains of calomel and ten of rhubarb were given, but without procuring a stool. In the afternoon he was bled ten ounces, and a purgative injection was ordered; after which his bowels were freely opened, and his pain subsided. An enema, consisting of a hundred drops of laudanum and two ounces of water, was administered, and he soon after

fell asleep. The weather, on the day of the operation and several days after, was very hot. The mercury of the thermometer in the patient's room stood at 86° Fahrenheit. Placed between the toes of the aneurismal limb, it rose to 88°; between those of the sound limb 90°; at both knees it stood at 92°. His pulse was 100 and tense.

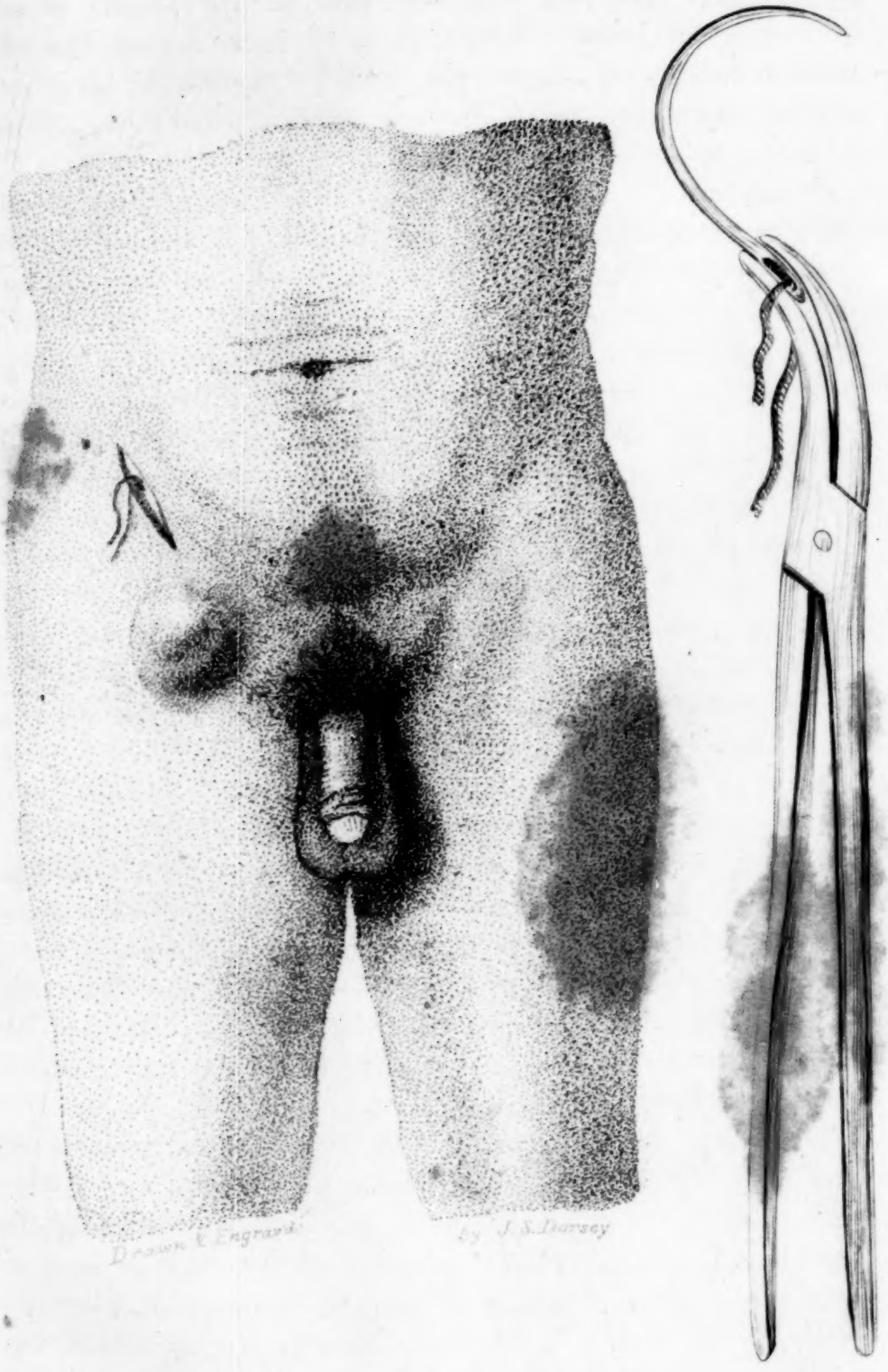
Wednesday 21st, *third day* after the operation. The sleep procured by the anodyne injection continued all night. In the fore part of the day he was easy; but in the evening his pain returned with considerable fever. He was bled ten ounces and took ten grains of magnesia and as much rhubarb: this with the assistance of a clyster, brought away a large quantity of fæces and flatus, and procured relief of all his pain. The anodyne injection was again administered, and he soon after slept. His pulse 100 and somewhat tense.

Thursday, *fourth day*. He slept all night, and was much better; being quite free from pain and fever. His pulse 90. He ate some boiled rice with great relish. The wound was examined, and it was found that nearly all of it had united: a little healthy pus surrounded the ligature. The limb was four degrees colder than the sound one.

From this time no change of importance occurred until Sunday, 1st Sept. when the ligature came away; viz. on the fourteenth day after the operation. In a few days more the wound cicatrized, without the occurrence of a single unpleasant symptom. On the twentieth day after the operation, his nurse being absent, he arose from bed and walked across the room, and has taken exercise every day since without inconvenience. The tumor in the groin diminishes slowly, and at this time is much reduced in size.

Remarks. The operation of tying up the external iliac artery above Poupart's ligament was first performed by Mr. Abernethy, under circumstances in which immediate death was the only alternative. He repeated it afterwards in cases of aneurism seated so high in the femoral artery as to preclude all prospect of a cure by any other means. He performed the operation four times. In the first two instances his patients

* Abernethy's Surgical Observations.



Drawn & Engraved by J. S. Harvey

INGUINAL ANEURISM.

died; in the succeeding cases they recovered.* Mr. Freer, in the Birmingham hospital, performed the operation in a case of inguinal aneurism with complete success; and soon after another case was treated successfully by Mr. Tomlinson, in the same hospital.† These six cases are all I have seen related, in which the operation has been tried. The case I have now detailed is the seventh; and it has failed in only two of these. In every instance the limb has been supplied with blood, which does not uniformly happen after the operation for popliteal aneurism.

I wish, before closing this paper, to call the attention of those surgeons, who may have occasion to perform the operation, to the forceps, an engraving of which is annexed. This instrument was contrived several years ago by Dr. Physick for the purpose of passing a needle under the pudic artery, when wounded in lithotomy, and has since been used by him for securing bleeding arteries in deep narrow wounds. Mr. Abernethy complains of "the great difficulty of turning a common needle in a deep narrow wound;" and Mr. Freer was unable to pass his aneurismal needle round the iliac artery, until he punctured the fascia surrounding it with his knife, which he confesses was the most "difficult and dangerous part of the operation." These dangers and difficulties are entirely obviated by means of the curved forceps; and I think the operation greatly simplified by the use of this contrivance.

Should this paper meet the eyes of Mr. Abernethy, I hope he will be gratified with the additional testimony of the importance of an operation for which the world is indebted to the intrepid efforts of true genius; and he will no doubt learn with pleasure, that one individual on this side the Atlantic owes to it, his life.

Arch Street, 30th September, 1811.

☞ *The sketch which accompanies this communication was intended to point out the part where the operation was performed, and the instrument used to convey the ligature round the artery. It needs no particular explanation.*

† Freer on Aneurism.

ORIGINAL REVIEW.

On the Morbid Sensibility of the Eye, commonly called Weakness of Sight. By John Stevenson, member of the Royal College of Surgeons, &c. London, 1810.

THE author informs us, in the introduction, that he has been frequently consulted by persons afflicted with this complaint, which has not, he believes, been hitherto described as a distinct disease, nor has its cure been established upon a rational pathology.

Many of these cases had been of long continuance, and had been treated with tonic remedies, on the supposition that the disease proceeded from debility. The author was for some time of this opinion; but the failure of success induced him to change his sentiments and practice. The following is his description of the disease.

By simply inspecting the eye, it is scarcely possible to recognise the existence of this complaint; as there is not the slightest external ophthalmia or unusual fulness of the vessels of the conjunctiva, any apparent affection of the ciliary glands, nor the least visible organic derangement. The characteristic symptoms are, a morbid sensibility of the eye to light, and to different kinds of external stimuli. A strong glare of light is always painfully distressing to the patient; and hence, in aggravated cases of this disease, the effulgence of the sun's rays, when admitted to the eye, excites in it a very acute sensation, which is accurately referred to the bottom of the orbit; around which there is at the same time, a sense of tension, and of oppressive uneasiness. For the same reason, the patient is miserably uncomfortable in a brilliantly illuminated apartment. In order therefore to exclude the strong and direct rays of light, he instinctively depresses his eyebrows, or applies his hand to his forehead, viewing objects with the palpebræ half closed; by which he is apt to acquire the habit of blinking. If he attempts to read, or look at small or bright objects, he is soon dazzled, and his vision becomes confused; which, added to the pain the effort occasions, speedily compels him to desist.

The iris acts with great energy on the admission of the rays of light to the retina, and, in consequence, the pupil becomes contracted to a very small aperture; a STRIKING FEATURE of this disease. When the stimulus of light affects the eye, there is sometimes, though very rarely, a manifestly deficient action of the lachrymal glands; but much oftener the secretion of tears is abundantly copious, which is indeed the principal cause of the confused vision occasionally attending this malady.

General debility, however induced, though not essential, seems greatly to *predispose* to this complaint. Hence its most frequent occurrence to persons recovering from previous illness, and to those of a relaxed habit. Although I have witnessed it in both sexes, yet the female, for the causes just specified, seems most obnoxious to its attacks; which may take place at all periods of life. Preceding ophthalmia, whether acute or chronic, gives the organ a great susceptibility to this disease; which is very apt to recur on the application of the exciting causes. These are undoubtedly, the long or frequent exposure of the eye to a very vivid or reflected light, and its excessive exertion in reading or viewing minute and dazzling objects. And with regard to the proximate cause of weakness of sight, instead of local debility, I will hazard the opinion, that it consists in an exquisite irritability and sensibility of the retina, the effect of a great turgescency of the vessels, or a chronic inflammation of that membrane or of the choroid.

Agreeably to this supposition, the indications of cure must consist, not in giving additional tone by the use of cold astringent applications, and internal strengthening medicines, but in lessening the plethoric state of the vessels of the posterior membranes of the eye, and in obviating, at the same time, the extreme sensibility of the retina.

The author accordingly directs the application of leeches to the lower eyelids; a few grains of calomel at bed time, to be purged off next morning with a solution of cream of tartar and emetic tartar; to foment the eyes morning and evening with an infusion of chamomile and poppy heads as hot as it can be comfortably borne; and immediately after being well dried, to drop into the internal canthi of the eyes the following Tinc-

tura opii mitis. Take of purified opium, of English saffron, each two drachms; white French Brandy, an ounce; distilled water seven ounces. Let them stand in a close vessel for six days, then filter for use. Apply frequently during the day a collyrium of cerussa acetata, made warm; wear a shade over the forehead, and adopt the antiphlogistic regimen. In a few days repeat the calomel and purging medicine. When the leeches cannot be applied, smart purging must be continued for some days. After the tenderness of the sight has been removed by these means, tonic collyria, purgative pills, and Peruvian bark are recommended to complete the cure. The collyria, preferred by the author, are solutions of white vitriol in water, either alone, or with a very small quantity of oil of vitriol and brandy: they should all be transparent, and applied cold.

Whenever the eye has been universally dry, accompanied with a slight degree of heaviness about the orbit, a snuff of powdered fox-glove has afforded considerable relief; tending most powerfully to unload the contiguous vessels of the eye. In a note, we are told, "In the lists of Errhines, I do not recollect to have seen the foxglove included; a herb which, I believe, is scarcely known to possess the peculiar property of that class of remedies. From experience, however, I find that when used in its active state, it excites a very copious excretion of thin mucus from the membrane lining the nostrils, without producing any narcotic effects.

"This fact was first communicated to me by an eminent physician in the country, who became accidentally acquainted with it, by observing an old woman using it as a substitute for common snuff."

In addition to the tonic remedies enumerated, we would mention one which we have found to be very powerful in cases of morbid sensibility of the eyes, from ophthalmia, and which we believe is seldom used. We allude to the air of the country, which has in several instances been found beneficial when all other remedies had proved ineffectual.

MEDICAL INTELLIGENCE.

Extract of a letter from Dr. David Hossack, Professor of the Theory and Practice of Medicine in the University of New-York, to one of the editors of the Eclectic Repertory, dated New-York, March 18, 1811.

"I will conclude this letter by stating to you, that on the first of February last I delivered a lady in this City, Mrs. C—, about twenty two years of age, who had not menstruated for near two years before her pregnancy. The suppression, which took place about eighteen months before her marriage, was the effect of cold, occasioned by getting her feet wet at the time her menses were flowing; but prior to that suppression, her catamenia had been regular, both as to quantity and the periods of their return. About six months before her marriage, she had a very small discharge of a blackish matter from the uterus; but so inconsiderable, that she states it to have been *a mere show*. Since her marriage, which took place in October 1809, she has not had the least return of her catamenia, either prior to or since her pregnancy. Her labour was in all respects natural, without any extraordinary discharge after the separation of the placenta, which also came away spontaneously. The lochial discharge continued the usual period; and she makes an excellent nurse, having plenty of milk for her child. This fact, of her proving pregnant after two years' absence of the catamenia, being very unusual, and contrary to the observations of the most respectable writers, I thought might prove interesting to you as a teacher of midwifery."

Vaccination in Glasgow.

The following statement contains perhaps one of the most decisive proofs of the utility of vaccination which has been submitted to the public. The first column contains the year. The second the number who have died of the small-pox in the city. The third the whole number of deaths in the city. The fourth the number of deaths in the city and suburbs.

First.	Second.	Third.	Fourth.
1792	403	1508	1912
1793	134	1356	2190
1794	278	1365	2445
1795	132	987	1700
1796	265	1327	2297
1797	134	961	1813
1798	231	1125	2064
1799	179	1025	2181
1800	224	1279	2499
1801	159	985	2096
1802	107	825	1928
1803	91	1158	2438
1804	123	1011	2224
1805	21	968	2389
1806	15	939	2280
1807	48	1002	2463
1808	14	1446	3265
1809	54	——	2368
1810	23	1121	2367

There are perhaps few towns of the same magnitude where the beneficial effects of vaccination have been more distinctly experienced. None of those jarring opinions which have disgraced other parts of the kingdom, are known in Glasgow. The profession universally recommend the practice, and the people almost as universally receive it. The few deaths by small-pox, which have occurred, within these last six years, have been exclusively among recent incomers, and the poorest and most wretched of the Highlanders and Irish. It appears from the records of the Vaccine Institution, that, previous to January, 1811, upwards of *fourteen thousand five hundred* have been inoculated *gratis*.—*Edin. Jour.*

Report of the Surgeons of the Vaccine Institution of Edinburgh, 1810.

In reporting to the managers of the Vaccine Institution the state of vaccination for 1810, the surgeons have the satisfaction

to mention, that nothing has occurred which can in any degree diminish their belief of the perfect efficacy of the cow-pox, as a preventive of the small-pox.

Since last report, 583 have been vaccinated, making in all 11,108, from the commencement of the practice at the Institution.

The surgeons have, since last report, inoculated with the small-pox a great many children, who have been vaccinated eight, nine, and ten years ago—all of whom have been found to resist the infection.

In no instance have they seen any bad effects which could be attributed to vaccination; and, upon the whole, the experience of another year serves to confirm them in their former opinion, that the practice of vaccination is deserving of the highest degree of confidence from the public.

Signed by WM. FARQUHARSON.
JAMES BRYCE.
ALEXR. GILLISPIE.
JOHN ABERCROMBIE.

Edinburgh, Jan. 21, 1811.

The resources with which Nature is provided for distributing the vital fluid through the bodies of animals, when the principal trunks of arteries are destroyed, has been remarkably exemplified in experiments lately made by Mr. Ashley Cooper. That gentleman tied the *aorta descendens* of dogs, very near to the heart, in a way to stop the current of blood passing, by that vessel, to all the lower parts of the frame. The animals seemed to sustain no great inconvenience by this: the wounds soon healed, the health was not impaired, the secretions proceeded as usual, and the creatures remained active and lively. When they were killed after some weeks, or months, for the purpose of ascertaining the changes that had happened, from the destruction of a part presumed to be so essential to life, the aorta was found obliterated where the ligature had been fixed, and the blood had been transmitted by anastomosing branches.

A marked instance of the appearance of Small-pox twice in the same person has occurred in the case of the Rev. Mr. Rowley, son of lady Rowley. About forty years ago Mr. Rowley, then a child, was inoculated for the Small-pox, by Mr. Adair, Surgeon general, and had a considerable eruption. On the 5th of June last, he was seized with fever, and an eruption appeared on the third day: there were two hundred pustules on the face, and the distemper has proved a severe case of distinct Small-pox.

Another instance of Small-pox after inoculation has happened to Miss S. Booth, of Covent Garden Theatre. At five years of age this young lady was inoculated for Small-pox. The progress of the arm was regular, she had considerable fever, and the whole of the appearances were of a nature to afford, it was believed, a perfect security from any future attack of the disease. On the 20th of June, she was seized with febrile symptoms, which proved the precursor of Small-pox: on Sunday, the 3d day from the attack, pustules appeared on the forehead and scalp. The eruption spread to other parts of the frame, accompanied with sore throat. This eruption passed through the usual forms and stages of the disease, and constituted an undoubted case of *Variola*.

A paper by Mr. Home has been read before the Royal Society, on the nonconducting powers of the thoracic duct and the spleen from the stomach to the bladder. Mr. H. related a number of experiments made with ligatures, or by removing the spleen, and keeping a ligature on the thoracic duct, in which state, rhubarb was taken into the stomach of the animal, and was detected in the urine; whence he inferred, contrary to some opinions he formerly published, that those two organs are not necessary to the secretion of urine.

Mr. Brodie has read before the same society a paper on the effects of vegetable poisons on animals. The author has pursued his researches for a considerable time, and detailed to the society the result of his experiments on rabbits, cats, and dogs, with alcohol, oil of bitter almonds, extract of aconite, tobacco, &c.

These vegetable substances thrown into the stomachs of dogs, cats, rabbits, &c. instantly killed them by acting on the nervous system, and producing a compression of the brain: thrown into the rectum, the same effects were produced. The pulsation and heat of the heart, after administering these poisons, were maintained for a considerable time by means of artificial respiration, except with tobacco, which instantly destroyed the powers of the heart, and arrested the pulsations. One drop of the empyreumatic oil of tobacco let fall on the tongue of a cat killed her, but did not destroy the pulsation so instantaneously. Mr. Brodie made a great number of experiments with the vegetable poison used by the American Indians to poison their arrows, and with nearly similar results.

We learn, that Dr. Rousseau of this city has ascertained by a series of original experiments, which were afterwards repeated in conjunction with Dr. Samuel Smith, that the effects of the effluvia of a variety of deleterious substances are not owing to absorption from the lungs into the circulation, but to an operation exclusively on the olfactory nerves. We are promised by these ingenious experimentalists, a communication on this subject for the next number of our journal.

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